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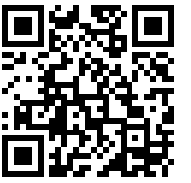

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# PROCEEDINGS

OF

# THE AMERICAN ASSOCIATION

FOR THE

# ADVANCEMENT OF SCIENCE,

TWENTY-SEVENTH MEETING,

HELD AT

## ST. LOUIS, MISSOURI,

AUGUST, 1878.

SALEM:
PUBLISHED BY THE PERMANENT SECRETARY.
1879.

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EDITED BY

FREDERICK W. PUTNAM,

*Permanent Secretary.*

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H. A. NEWTON, of New Haven,
BENJAMIN PEIRCE, of Cambridge,
WILLIAM B. ROGERS, of Boston,
J. LAWRENCE SMITH, of Louisville,
E. B. ELLIOTT, of Washington.

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*From the Association at large.*—Six Fellows to be elected on the first day of the meeting.



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FOR THE

# SARATOGA MEETING.

---

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## MEETINGS AND OFFICERS OF THE AMERICAN ASSOCIATION OF GEOLOGISTS AND NATURALISTS.

| MEETING. | DATE. | PLACE. | CHAIRMAN. | SECRETARY. | ASSIST. SEC'Y. | TREASURER. |
|---|---|---|---|---|---|---|
| 1st | April 2, 1840, | Philadelphia, | Edward Hitchcock,* | L. C. Beck,* | | |
| 2d | April 5, 1841, | Philadelphia, | Benjamin Silliman,* | L. C. Beck,* | B. Silliman, jr., C. B. Trego, | |
| 3d | April 25, 1842, | Boston, | S. G. Morton,* | C. T. Jackson, | J. D. Whitney, M. B. Williams, | |
| 4th | April 26, 1843, | Albany, | Henry D. Rogers,* | B. Silliman, jr., | | John Locke.* |
| 5th | May 8, 1844, | Washington, | John Locke,* | B. Silliman, jr., O. P. Hubbard, | | Douglas Houghton.* |
| 6th | April 30, 1845, | New Haven, | Wm. B. Rogers, | B. Silliman, jr., J. Lawrence Smith, | | Douglas Houghton.* |
| 7th | Sept. 2, 1846, | New York, | C. T. Jackson, | B. Silliman, jr., | | E. C. Herrick.* |
| 8th | Sept. 20, 1847, | Boston, | Wm. B. Rogers,† | Jeffries Wyman,* | | B. Silliman, jr. |

*Deceased.

†Professor ROGERS, as chairman of this last meeting, called the first meeting of the new Association to order and presided until it was fully organized by the adoption of a constitution. As he was thus the first presiding officer of the new Association, it was directed at the Hartford meeting that his name be placed at the head of the Past Presidents of the American Association for the Advancement of Science.



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## MEETINGS AND OFFICERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

| MEETING. | DATE. | PLACE. | PRESIDENT. | VICE-PRESIDENT. | GENERAL SECRETARY. | PERMANENT SEC'Y. | TREASURER. |
|---|---|---|---|---|---|---|---|
| 1st | Sept. 20, 1848. | Philadelphia, Pa.. | W. C. Redfield.* | | Walter R. Johnson,* | | Jeffries Wyman.* |
| 2d | Aug. 14, 1849, | Cambridge, Mass., | Joseph Henry,* | | E. N. Horsford, 1 | | A. L. Elwyn. |
| 3d | Mar. 12, 1850. | Charleston, S. C., | A. D. Bache,* 2 | | L. R. Gibbs, 3 | | St. J. Ravenel.* 4 |
| 4th | Aug. 19, 1850. | New Haven, Conn., | A. D. Bache,* | | E. C. Herrick,* | | A. L. Elwyn. |
| 5th | May 5, 1851. | Cincinnati, Ohio, | A. D. Bache,* | | W. B. Rogers, 5 | S. F. Baird, | S. F. Baird. 6 |
| 6th | Aug. 19, 1851. | Albany, N. Y., | Louis Agassiz,* | | W. B. Rogers, | S. F. Baird, | A. L. Elwyn. |
| 7th | July 28, 1853. | Cleveland, Ohio, | Benjamin Pierce, | | S. St. John, 7 | S. F. Baird, | A. L. Elwyn. |
| 8th | April 26 1854. | Washington, D.C.. | J. D. Dana, | | J. Lawrence Smith, | Joseph Lovering, | J. L. LeConte. 8 |
| 9th | Aug. 15, 1855. | Providence, R. I., | John Torrey,* | | Wolcott Gibbs, | Joseph Lovering, | A. L. Elwyn. |
| 10th | Aug. 20, 1856. | Albany, N. Y., | James Hall, | | B. A. Gould, | Joseph Lovering, | A. L. Elwyn. |
| 11th | Aug. 12, 1857. | Montreal, Canada. | Alexis Caswell,* 9 | Alexis Caswell,* | John LeConte, | Joseph Lovering, | A. L. Elwyn. |
| 12th | April 28, 1858. | Baltimore, Md., | Alexis Caswell,* 10 | John E. Holbrook,*† | W. M. Gillespie,* 11 | Joseph Lovering, | A. L. Elwyn. |
| 13th | Aug. 3, 1859. | Springfield, Mass., | Stephen Alexander, | Edward Hitchcock,* | William Chauvenet,* | Joseph Lovering, | A. L. Elwyn. |
| 14th | Aug. 1, 1860. | Newport, R. I., | Isaac Lea, | B. A. Gould, | Joseph LeConte, | Joseph Lovering, | A. L. Elwyn. |
| 15th | Aug. 15, 1866. | Buffalo, N. Y., | F. A. P. Barnard, | A. A. Gould,* 12 | Elias Loomis, 13 | Joseph Lovering, | A. L. Elwyn.† |
| 16th | Aug. 21, 1867. | Burlington, Vt., | J. S. Newberry, | Wolcott Gibbs, | C. S. Lyman, | Joseph Lovering, | A. L. Elwyn.† |
| 17th | Aug. 5, 1868. | Chicago, Ill., | B. A. Gould, | Charles Whittlesey, | Simon Newcomb, 14 | Joseph Lovering, | A. L. Elwyn.† |
| 18th | Aug. 18, 1869. | Salem, Mass., | J. W. Foster,* | O. N. Rood, | O. C. Marsh, | F. W. Putnam, 15 | A. L. Elwyn.† |
| 19th | Aug. 17, 1870. | Troy, N. Y., | T. S. Hunt, 16 | T. S. Hunt, | F. W. Putnam, 17 | Joseph Lovering, | A. L. Elwyn.† |
| 20th | Aug. 16, 1871. | Indianapolis, Ind.. | Asa Gray, | G. F. Barker, | F. W. Putnam, | Joseph Lovering, | W. S. Vaux. |
| 21st | Aug. 15, 1872 | Dubuque, Iowa, | J. Lawrence Smith, | Alex. Winchell, | E. S. Morse, | Joseph Lovering. | W. S. Vaux. |
| 22d | Aug. 20, 1873 | Portland, Me., | Joseph Lovering. | A. H. Worthen.† | C. A. White, | F. W. Putnam, | W. S. Vaux. |
| 23d | Aug. 12, 1874. | Hartford, Conn.. | J. L. LeConte, | C. S. Lyman. | A. C. Hamlin, | F. W. Putnam. | W. S. Vaux.† |

1. In place of Jeffries Wyman, *not present.*
2. In place of Joseph Henry, *not present.*
3. In place of E. C. Herrick, *not present.*
4. In place of A. L. Elwyn, *not present.*
5. In place of E. C. Herrick, *not present.*
6. In place of **A. L. Elwyn**, *not present.*
7. In place of J. D. Dana, *not present.*
8. In place of A. L. Elwyn, *not present.*
9. In place of J. W. Bailey, *deceased.*
10. In place of Jeffries Wyman, *not present.*
11. In place of Wm. Chauvenet, *not present.*
12. In place of R. W. Gibbes, *not present.*
13. In place of W. P. Trowbridge, *not present.*
14. In place of A. P. Rockwell, *called home 1st day.*
15. In place of Joseph Lovering, *in Europe.*
16. In place of Wm. Chauvenet, *too ill to be present.*
17. In place of C. F. Hartt, *in Brazil.*

* Deceased. † Not present at the meeting.

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## MEETINGS AND OFFICERS OF THE ASSOCIATION (*Continued*).

| MEETING. | DATE. | PLACE. | PRESIDENT. | VICE PRESIDENT, SECTION A. |
|---|---|---|---|---|
| 24th | Aug. 11, 1875, | Detroit, Mich., | J. E. Hilgard, | H. A. Newton, |
| 25th | Aug. 23, 1876, | Buffalo, N. Y., | William B. Rogers, | Charles A. Young, |
| 26th | Aug. 29, 1877, | Nashville, Tenn., | Simon Newcomb, | R. H. Thurston,[1] |
| 27th | Aug. 21, 1878. | St. Louis, Mo. | O. C. Marsh. | R. H. Thurston. |

| VICE PRESIDENT, SECTION B. | CHAIRMAN OF PERMANENT SUBSECTION C, CHEMISTRY. | CHAIRMAN OF PERMANENT SUBSECTION D, ANTHROPOLOGY. | CHAIRMAN OF PERMANENT SUBSECTION E, MICROSCOPY. |
|---|---|---|---|
| J. W. Dawson, | S. W. Johnson, | L. H. Morgan, | —— —— |
| Edward S. Morse, | George F. Barker, | L. H. Morgan, | R. H. Ward, |
| O. C. Marsh, | N. T. Lupton, | Daniel Wilson,* | R. H. Ward, |
| Aug. R. Grote. | F. W. Clarke. | ——— ———[2] | R. H. Ward.[3] |

| PERMANENT SECRETARY. | GENERAL SECRETARY. | SECRETARY OF SECTION A. | SECRETARY OF SECTION B. |
|---|---|---|---|
| F. W. Putnam, | S. H. Scudder, | S. P. Langley, T. C. Mendenhall, | E. S. Morse, |
| F. W. Putnam, | T. C. Mendenhall, | A. W. Wright, | Albert H. Tuttle, |
| F. W. Putnam, | Aug. R. Grote, | H. C. Bolton, | Wm. H. Dall, |
| F. W. Putnam. | H. C. Bolton. | F. E. Nipher. | George Little. |

| SECRETARY OF PERMANENT SUBSECTION C, CHEMISTRY. | SECRETARY OF PERMANENT SUBSECTION D, ANTHROPOLOGY. | SECRETARY OF PERMANENT SUBSECTION E, MICROSCOPY. | TREASURER. |
|---|---|---|---|
| F. W. Clarke, | F. W. Putnam, | —— —— | W. S. Vaux. |
| H. C. Bolton, | O. T. Mason, | E. W. Morley, | W. S. Vaux. |
| P. Schweitzer, | —— —— | T. O. Summers, jr., | W. S. Vaux.* |
| A. P. S. Stuart. | ——— ———[2] | Geo. J. Engelmann. | W. S. Vaux.* |

[1] In the absence of E. C. Pickering.
[2] The Subsection united with Sec. B.
[3] In the absence of G. S. Blackie.
* Not present.

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# COMMONWEALTH OF MASSACHUSETTS.

## IN THE YEAR ONE THOUSAND EIGHT HUNDRED AND SEVENTY-FOUR.

# AN ACT

### To Incorporate the "American Association for the Advancement of Science."

*Be it enacted by the Senate and House of Representatives, in General Court assembled, and by the authority of the same, as follows:*

Section 1. Joseph Henry of Washington, Benjamin Pierce of Cambridge, James D. Dana of New Haven, James Hall of Albany, Alexis Caswell of Providence, Stephen Alexander of Princeton, Isaac Lea of Philadelphia, F. A. P. Barnard of New York, John S. Newberry of Cleveland, B. A. Gould of Cambridge, T. Sterry Hunt of Boston, Asa Gray of Cambridge, J. Lawrence Smith of Louisville, Joseph Lovering of Cambridge and John LeConte of Philadelphia, their associates, the officers and members of the Association, known as the "American Association for the Advancement of Science," and their successors, are hereby made a corporation by the name of the "American Association for the Advancement of Science," for the purpose of receiving, purchasing, holding and conveying real and personal property, which it now is, or hereafter may be possessed of, with all the powers and privileges, and subject to the restrictions, duties and liabilities set forth in the general laws which now or hereafter may be in force and applicable to such corporations.

Section 2. Said corporation may have and hold by purchase, grant, gift or otherwise, real estate not exceeding one hundred thousand dollars in value, and personal estate of the value of two hundred and fifty thousand dollars.

Section 3. Any two of the corporators above named are hereby authorized to call the first meeting of the said corporation in the month of August next ensuing, by notice thereof "by mail," to each member of the said Association.

Section 4. This act shall take effect upon its passage.

House of Representatives, March 10, 1874.
Passed to be enacted,
John E. Sanford, *Speaker.*

In Senate, March 17, 1874.
Passed to be enacted,
Geo. B. Loring, *President.*

March 19, 1874.
Approved,
W. B. Washburn.

Secretary's Department,
Boston, April 3, 1874.
A true copy, Attest:
David Pulsifer,
Deputy Secretary of the Commonwealth.



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# CONSTITUTION

OF THE

# AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

**Incorporated by Act of the General Court of the Commonwealth of Massachusetts.**

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### OBJECTS.

ARTICLE 1. The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of America, to give a stronger and more general impulse and more systematic direction to scientific research, and to procure for the labors of scientific men increased facilities and a wider usefulness.

### MEMBERS, FELLOWS, PATRONS AND HONORARY FELLOWS.

ART. 2. The Association shall consist of Members, Fellows, Patrons and Honorary Fellows.

ART. 3. Any person may become a Member of the Association upon recommendation in writing by two members or fellows, nomination by the Standing Committee, and election by a majority of the members and fellows present in general session.

ART. 4. Fellows shall be nominated by the Standing Committee from such of the members as are professionally engaged in science, or have by their labors aided in advancing science. The election of fellows shall be by ballot and a majority vote of the members and fellows present in general session. But all persons who may be members at the time of the adoption of this constitution may become fellows by signifying their desire to this effect before the first day of August, 1875.

ART. 5. Any person paying to the Association the sum of one thousand dollars shall be classed as a Patron, and shall be entitled to all the privileges of a member and to all its publications.



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ART. 6. Honorary Fellows of the Association, to the number of ten for each section, may be elected; the nominations to be made by the Standing Committee and approved by ballot in the respective sections before election by ballot in general session. Honorary Fellows shall be entitled to all the privileges of fellows and shall be exempt from all fees and assessments, and entitled to all publications of the Association issued after the date of their election.

ART. 7. The name of any member or fellow two years in arrears for annual dues shall be erased from the list of the Association, provided that two notices of indebtedness, at an interval of at least three months, shall have been given; and no such person shall be restored until he has paid his arrearages or has been reëlected.

ART. 8. No member or fellow shall take part in the organization or business of both sections at the same meeting.

## OFFICERS.

ART. 9. The Officers of the Association shall be elected by ballot in general session from the fellows and shall consist of a President, two Vice Presidents, a General Secretary, a Permanent Secretary, a Treasurer, a Secretary of Section A, and a Secretary of Section B; these, with the exception of the Permanent Secretary, shall be elected at each meeting for the following one, and, with the exception of the Treasurer and the Permanent Secretary, shall not be reëligible for the next two meetings. The Permanent Secretary shall be elected at each fifth meeting.

ART. 10. The President, or, in his absence, one of the Vice Presidents, shall preside at all general sessions of the Association and at all meetings of the Standing Committee. It shall also be the duty of the President to give an address at a general session of the Association at the meeting following that over which he presided.

ART. 11. The Vice Presidents shall be the presiding officers of Sections A and B, and of the Sectional Committees, and it shall be part of their duty to give an address, each before his respective section, at such time as the section shall determine. The Vice Presidents may request their respective sections to appoint temporary chairmen to preside over the sessions of the sections, but shall not delegate their other duties.

ART. 12. The General Secretary shall be the Secretary of all general sessions of the Association, and of all sessions of the Standing Committee, and shall keep a record of the business of these sessions. He

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shall receive the records from the Secretaries of the Sections, which, after examination, he shall transmit with his own records to the Permanent Secretary within two weeks after the adjournment of the meeting. He shall receive proposals for membership and bring them before the Standing Committee. He shall give to the Secretary of each Section the list of papers assigned to it by the Standing Committee.

ART. 13. The Permanent Secretary shall be the executive officer of the Association under the direction of the Standing Committee. He shall attend to all business not specially referred to committees nor otherwise constitutionally provided for. He shall keep an account of all business that he has transacted for the Association, and make annually at the first meeting of the Standing Committee, a report which shall be laid before the Association. He shall attend to the printing and distribution of the annual volume of Proceedings, and all other printing ordered by the Association. He shall issue a circular of information to members and fellows at least four months before each meeting, and shall, in connection with the Local Committee, make all necessary arrangements for the meetings of the Association. He shall provide the Secretaries of the Association with such books and stationery as may be required for their records and business, and shall provide members and fellows with such blank forms as may be required for facilitating the business of the Association. He shall collect all assessments and admission fees, and notify members and fellows of their election, and of any arrearages. He shall receive, and bring before the Standing Committee, the titles and abstracts of papers proposed to be read before the Association. He shall keep an account of all receipts and expenditures of the Association, and report the same annually at the first meeting of the Standing Committee, and, at the close of each year, shall pay over to the Treasurer such unexpended funds as the Standing Committee may direct. He shall receive and hold in trust for the Association all books, pamphlets and manuscripts belonging to the Association, and allow the use of the same under the provisions of the Constitution and the orders of the Standing Committee. He shall receive all communications addressed to the Association during the interval between meetings, and properly attend to the same. He shall at each meeting report the names of fellows and members who have died since the preceding meeting. He shall be allowed a salary which shall be determined by the Standing Committee, and may employ a clerk at such compensation as may be agreed upon by the Standing Committee.

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ART. 14. The Treasurer shall invest the funds received by him in such securities as may be directed by the Standing Committee. He shall annually present to the Standing Committee an account of the funds in his charge. No expenditure of the principal in the hands of the Treasurer shall be made without a unanimous vote of the Standing Committee, and no expenditure of the income received by the Treasurer shall be made without a two-thirds vote of the Standing Committee.

ART. 15. The Secretaries of Sections A and B shall keep the records of their respective sections, and, at the close of the meeting, give the same, including the records of subsections, to the General Secretary. They shall also be the secretaries of the sectional committees.

ART. 16. In case of a vacancy in the office of the President, one of the Vice Presidents shall be elected by the Standing Committee as the President of the meeting. Vacancies in the offices of Vice President, General Secretary, Permanent Secretary and Treasurer, shall be filled by nomination of the Standing Committee and election by ballot in general session. A vacancy in the office of Secretary of a Section shall be filled by nomination and election by ballot in the section.

ART. 17. The Standing Committee shall consist of the past Presidents, the President, the Vice Presidents, the four Secretaries, the Treasurer, with the above named officers of the preceding meeting, and six fellows elected by ballot after open nomination at the first general session. The members present at any regularly called meeting of the Committee, provided there are at least five, shall form a quorum for the transaction of business. The Standing Committee shall meet on the day preceding each annual meeting of the Association, and arrange the programme for the first day of the sessions. The time and place of this first meeting shall be designated by the Permanent Secretary. Unless otherwise agreed upon, regular meetings of the Committee shall be held in the committee room at 9 o'clock, A.M., on each day of the meeting of the Association. Special meetings of the Committee may be called at any time by the President. The Standing Committee shall be the board of supervision of the Association, and no business shall be transacted by the Association that has not first been referred to, or originated with, the Committee. The special business of the Committee shall be: to receive and assign papers to the respective sections; to examine and, if necessary, to exclude papers; to decide which papers, discussions and other proceedings shall be published, and to have the general direction of

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the publications of the Association; to manage the financial affairs of the Association; to arrange the business and programmes for general sessions; to appoint general sessions for the evening; to suggest subjects for discussion, investigation or reports; to nominate members and fellows; to receive and act upon all invitations extended to the Association and report the same at a general session of the Association.

ART. 18. The Nominating Committee shall consist of the Standing Committee, and four members or fellows elected by each of the sections. It shall be the duty of this Committee to meet at the call of the President and nominate the general officers for the following meeting of the Association. It shall also be the duty of this Committee to recommend the time and place for the next meeting. The Vice Presidents and Secretaries of the Sections shall be recommended to the Nominating Committee by sub-committees consisting of the Vice Presidents and Secretaries, and the four persons elected by each section under the first clause of this article.

### MEETINGS.

ART. 19. The Association shall hold public meetings annually, for one week or longer, at such time and place as may be determined by vote of the Association, and the preliminary arrangements for each meeting shall be made by the Local Committee, in conjunction with the Permanent Secretary and such other persons as the Standing Committee may designate.

ART. 20. General Sessions shall be held at 10 o'clock, A. M., unless otherwise ordered, on every day of the meeting, Sunday excepted, and at such other times as may be appointed by the Standing Committee.

### SECTIONS AND SUBSECTIONS.

ART. 21. The Association shall be divided into two Sections, namely: **A** (*Mathematics*, *Astronomy*, *Physics*, *Chemistry* and *Mineralogy*) and **B** (*Geology*, *Zoology*, *Botany* and *Anthropology*). Either Section may, at its pleasure, form temporary or permanent subsections for the reading of papers.

ART. 22. Immediately on the organization of a Section there shall be three fellows elected by ballot after open nomination, who, with the Vice President and Secretary, and the Chairman and Secretary of the subsections, shall form its Sectional Committee. The Sectional Com-

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mittees shall have power to fill vacancies in their own numbers. There shall be no sectional meeting during a general session.

ART. 23. When any Subsection organizes, it shall elect a Chairman and Secretary and report the result to the Secretary of its Section. The Secretary of a Subsection shall, at the close of the meeting, transmit his records to the Secretary of the Section. Any Permanent Subsection may elect its Chairman for the ensuing meeting.

ART. 24. No paper shall be read in any Section or Subsection until it has been placed on the programme of the day by the Sectional Committees.

### SECTIONAL COMMITTEES.

ART. 25. The Sectional Committees shall arrange and direct the business of their respective sections. They shall prepare the daily programmes and give them to the Permanent Secretary for printing at the earliest moment practicable. No titles of papers shall be entered on the daily programmes except such as have passed the Standing Committee. No change shall be made in the programme for the day without the consent of the Sectional Committee. The Sectional Committees may refuse to place the title of any paper on the programme; but every such title, with the abstract of the paper or the paper itself must be returned to the Standing Committee with the reasons why it was refused.

ART. 26. The Sectional Committees shall examine all papers and abstracts referred to the sections, and they shall not place on the programme any paper inconsistent with the character of the Association; and to this end they have power to call for any paper, the character of which may not be sufficiently understood from the abstract submitted.

### PAPERS AND COMMUNICATIONS.

ART. 27. All members and fellows must forward to the Permanent Secretary, as early as possible, and when practicable before the convening of the Association, full titles of all the papers which they propose to present during the meeting, with a statement of the time that each will occupy in delivery, and also such abstracts of their contents as will give a general idea of their nature; and no title shall be referred by the Standing Committee to the Sectional Committee until an abstract of the paper or the paper itself has been received.

ART. 28. If the author of any paper be not ready at the time assigned, the title may be dropped to the bottom of the list.

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### Reports of Meetings.

Art. 29. Whenever practicable, the proceedings and discussions at general sessions, sections and subsections shall be reported by professional reporters, but such reports shall not appear in print as the official reports of the Association unless revised by the secretaries.

### Printed Proceedings.

Art. 30. The Permanent Secretary shall have the Proceedings of each meeting printed in an octavo volume as soon after the meeting as possible, beginning one month after adjournment. Authors must prepare their papers ready for the press and forward them to the Permanent Secretary within this interval, otherwise only the abstracts or titles will appear in the printed volumes. The Standing Committee shall have power to print an abstract only of any paper. Whenever practicable, proofs shall be forwarded to authors for revision. If any additions or substantial alterations are made by the author of a paper after its submission to the Secretary, the same shall be distinctly indicated. Illustrations must be provided for by the authors of the papers, or by a special appropriation from the Standing Committee. Immediately on publication of the volume, a copy shall be forwarded to every member and fellow of the Association who shall have paid the assessment for the meeting to which it relates, and it shall also be offered for sale by the Permanent Secretary at such price as may be determined by the Standing Committee. The Standing Committee shall also designate the institutions to which copies shall be distributed.

### Local Committee.

Art. 31. The Local Committee shall consist of persons interested in the objects of the Association and residing at or near the place of the proposed meeting. It is expected that the Local Committee, assisted by the officers of the Association, will make all essential arrangements for the meeting, and issue a circular giving necessary particulars, at least one month before the meeting.

### Library of the Association.

Art. 32. All books and pamphlets received by the Association shall be in the charge of the Permanent Secretary, who shall have a list of the same printed and shall furnish a copy to any member or fellow on application. Members and fellows who have paid their assessments in full

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shall be allowed to call for books and pamphlets, which shall be delivered to them at their expense, on their giving a receipt agreeing to make good any loss or damage and to return the same free of expense to the Secretary at the time specified in the receipt given. All books and pamphlets in circulation must be returned at each meeting, or the value of any not so returned paid to the Permanent Secretary. Not more than ten books, including volumes, parts of volumes, and pamphlets, shall be held at one time by any member or fellow. Any book may be withheld from circulation by order of the Standing Committee.

### Admission Fee and Assessments.

Art. 33. The admission fee for members shall be five dollars in addition to the annual assessment. On the election of any member as a fellow an additional fee of two dollars shall be paid.

Art. 34. The annual assessment for members and fellows shall be three dollars.

Art. 35. Any member or fellow who shall pay the sum of fifty dollars to the Association, at any one time, shall be exempt from all further assessments, but this payment shall not entitle him to the publications of the Association, and all money thus received shall be invested as a permanent fund, the income of which shall be used only to assist in original research.

Art. 36. All admission fees and assessments must be paid to the Permanent Secretary, who shall give proper receipts for the same.

### Accounts.

Art. 37. The accounts of the Permanent Secretary and of the Treasurer shall be audited annually, by two auditors appointed by the Standing Committee.

### Alterations of the Constitution.

Art. 38. No part of this Constitution shall be amended or annulled, without the concurrence of three-fourths of the members and fellows present in general session, after notice given at a general session of a preceding meeting of the Association.

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# ORDER OF PROCEEDINGS

IN

# ORGANIZING A MEETING.

1. The retiring President introduces the President elect, who takes the chair.
2. Formalities of welcome of the Association as may be arranged by the Local Committee.
3. Report of the list of papers on the register and their reference to the Sections.
4. Other reports.
5. Announcements of arrangements by the Local Committee.
6. Elections to complete the Standing Committee.
7. Election of members.
8. Election of fellows.
9. Unenumerated business.
10. Adjournment to meet in Sections.

This order, so far as applicable, to be followed in subsequent General Sessions.



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# MEMBERS

OF THE

# AMERICAN ASSOCIATION

FOR THE

# ADVANCEMENT OF SCIENCE.[1]

---

## PATRON.[2]

Thompson, Mrs. Elizabeth, Stamford, Conn. (22). (Entitled to annual volume.)

## MEMBERS.[3]

Abbot, Miss Elizabeth O., 16 Clark St., Cincinnati, Ohio (20).
Acheson, Edward G. (25).
Aikin, Prof. W. E. A., Baltimore, Md. (12).
Albert, Augustus J., Baltimore, Md. (12).
Alexander, Prof. Stephen, Princeton, N. J. (1).
Allen, Edmund T., 419 Olive St., St. Louis, Mo. (27).
Allen, Gerard B., cor. 2nd and Carr streets, St. Louis, Mo. (27).
Allen, J. M., Hartford, Conn. (22).
Allen, Hon. Thomas, St. Louis, Mo. (27).
Ammen, Rear Admiral Daniel, U. S. Navy, Beltsville, Md. (26).
Armsby, Henry P., Box 495, New Haven, Conn. (23).
Arthur, J. C., Charles City, Iowa. (21).
Atchison, Prof. Thos. A., University of Nashville, Nashville, Tenn. (26.)
Atkinson, Jno. B., Earlington, Hopkins Co., Ky. (26).
Atwell, W. H., Care Atwell and Sneed, Nashville, Tenn. (26).

[1] The numbers in parentheses indicate the meeting at which the member was elected. The Constitution requires that the names of all members two or more years in arrears shall be omitted from the list, but their names will be restored on payment of arrearages. Members not in arrears are entitled to the annual volume of Proceedings.

[2] Persons contributing one thousand dollars or more to the Association are classed as Patrons, are entitled to the privileges of members and to the publications.

[3] Any Member or Fellow may become a Life Member by the payment of fifty dollars. The money derived from Life Memberships is invested as a fund, the income of which is to be used only to aid in original research. Life Members are exempt from the annual assessment, and are entitled to the annual volume. The names of Life Members are printed in small capitals in the regular list of Members and Fellows.



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Austen, Peter T., Ph. D., Rutger's College, Lock Box No. 2, New Brunswick, N. J. (26).

Bailey, E. H. S., Bethlehem, Pa. (25).
Baird, Alexander J., D.D., Nashville, Tenn. (26).
Baker, Prof. T. R., Millersville, Lancaster Co., Pa. (22).
Bandelier, Ad. F., Highland, Ill. (25).
Barnard, Edward E., care R. Poole, Cherry and Union Sts., Nashville, Tenn. (26).
Barnard, Jas. M., Boston, Mass. (18).
Barnes, Charles J., Chicago, Ill. (27).
Barnett, Prof. Samuel, jr., Washington, Wilkes Co., Ga. (26).
Barry, Sir Redmond, President Public Library of Melbourne, Victoria, Australia (25).
Barstow, Charles W., St. Louis, Mo. (27).
Bassett, George W., M.D., Vandalia, Ill. (20).
Bassnett, Thomas, Jacksonville, Fla. (8).
Bassnett, Mrs. Thomas, Jacksonville, Fla. (24).
Bastian, Dr. David I., Clinton, Worcester Co., Mass. (25).
Bate, Gen. Wm. Brimage, Nashville, Tenn. (26).
Batterson, J. G., Hartford, Conn. (23).
Bausch, Edward, Rochester, N. Y. (26).
Beach, Charles M., Merchant, Hartford, Conn. (23).
Beach, J. Watson, Merchant, Hartford, Conn. (23).
Beach, William H., Beloit, Wis. (21).
Beal, Prof. Wm. James, Agricultural College, Lansing, Mich. (24).
Becker, Hon. Philip, Buffalo, N. Y. (25).
Bell, Prof. Alex. Graham, Boston University, Boston, Mass. (26).
Bell, John J., Exeter, N. H. (22).
Bemis, Hon. A. S., President of the Common Council, Buffalo, N. Y. (25).
Benjamin, E. B., 10 Barclay St., New York (19).
Benjamin, Marcus, 10 Barclay St., New York (27).
Bessels, Dr. Emil, Smithsonian Institution, Washington, D. C. (26).
Bessey, Prof. C. E., Agricultural College, Ames, Iowa (21).
Bickmore, Prof. Albert S., Am. Mus. of Nat. Hist., Central Park, N. Y. (17).
Bill, Charles, Springfield, Mass. (17).
Blackham, Dr. George E., Dunkirk, N. Y. (25).
Blair, Henry Wayne, U. S. Coast Survey, Lexington, Va. (26).
Blatchford, Eliphalet W., 375 North La Salle St., Chicago, Ill. (17).
Bohrer, Charles J., Lafayette, Ind. (25).
Bolles, Rev. E. C., Salem, Mass. (17).
Bowen, Edmund S., General Supt. Erie Railway, New York (25).
Bowers, Miss Virginia K., Grand Eddy P. O., Perry Co., Mo. (27).
Bowles, Miss Margaretta, P. O. Box 25, Louisville, Ky. (26).
Braden, John, D.D., Pres. Central College, Nashville, Tenn. (26).
Bradley, C. W., 104 North 4th St., St. Louis, Mo. (27).

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Breckinridge, S. M., P. O. Box 2794, St. Louis, Mo. (27).
Breneman, A. A., Ithaca, N. Y. (20).
Briggs, Albert D., 7 Fort Block, Springfield, Mass. (13).
Briggs, Charles S., Vanderbilt University, Nashville, Tenn. (26).
Broadhead, Garland Carr, Pleasant Hill, Cass Co., Mo. (27).
Brooks, Major Thos. B., Geologist State Surveys Mich. and Wis., Newburgh, N. Y. (26).
Broomall, Hon. John M., Media, Delaware Co., Pa. (23).
Brown, Mrs. Ashley, 115 Wilkinson St., Dayton, Ohio (25).
Brown, Horace, Connsellor at Law, Salem, Mass (27).
Brown, Hon. John C., Vice President, Texas and Pacific R. R. Co., Pulaski, Tenn. (26).
Brown, Rev. John Reese, D.D., Nashville, Tenn. (26).
Brush, Dr. Edward N., Buffalo N. Y. (25).
Bryan, Malachi Thomas, Nashville, Tenn. (26).
Bryan, Oliver N., Accokeek P. O., Prince George's Co., Md. (18).
Buckhout, W. A., State College, Centre Co., Pa. (20).
Buist, John Robinson, M.D., Pres. Nashville Board of Health, Nashville, Tenn. (26).
Bull, Dr. Alexander T., 98 Swan St., Buffalo, N. Y. (25).
Bush, Stephen, Waterford, N. Y. (19).
Butters, Alexander, Carlinville P. O., Ill. (27).

Cairns, F. A., School of Mines, New York (27).
Caldwell, John W., M.D., Stewart College, Clarksville, Tenn. (26).
Calthrop, Rev. S. R., Syracuse, N. Y. (25).
Campbell, A. C., Nashville, Tenn. (26).
Campbell, Mrs. Mary H., Crawfordsville, Ind. (22).
Campbell, Dr. M., Nashville, Tenn. (27).
Canfield, Rev. Eli H., D.D., Arlington, Bennington Co., Vt. (25).
Capen, Miss Bessie T., Smith College, Northampton, Mass. (23).
Carpenter, Prof. G. C., Simpson Centenary College, Indianola, Iowa (22).
Carrington, Col. Henry B., U.S.A., Prof. in Wabash College, Crawfordsville, Ind. (20).
Case, Leonard, Cleveland, Ohio (15).
Cassino, S. E., Naturalists' Agency, Salem, Mass. (25).
Caswell, John H., 11 West 48th Street, New York. (26).
Cattell, William C., President Lafayette College, Easton, Pa. (15).
Chandler, Henry, Lewis Block, Buffalo, N. Y. (25).
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Schanck, Prof. J. Stillwell, Princeton, New Jersey (4).
Schley, William S., Salem, Lee Co., Ala. (26).
Schmelter, Henry, P. O. Box 2135, New York (25).
Schmid, Dr. H. Ernest, White Plains, N. Y. (25).
Scobey, James E., Pres. Murfreesboro Fem. Inst., Murfreesboro, Tenn. (26).

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Seaman, Ezra C., Ann Arbor, Mich. (20).
Seeley, Charles A., 26 Pine St., New York (18).
Seymour, Prof. William P., 105 Third St., Troy, N. Y. (19).
Sharp, Prof. Solomon Z., Ashland, Ohio (26).
Shaub, Julius W., St. Louis, Mo. (27).
Shaw, Henry, St. Louis, Mo. (27).
Sheafer, P. W., Pottsville, Pa. (4).
Sheafer, Walter S., Pottsville, Pa. (25).
Sheldon, Edwin H., Chicago, Ill. (17).
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Skinner, Joseph J., New Haven, Conn. (23).
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Smith, J. W., M.D., Charles City, Iowa (21).
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Smith, Mrs. Sarah J., (26).
Smith, William Austin, M. D., Columbia, Tenn. (26).
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Snyder, Prof. Monroe B., High School Observatory, Philadelphia, Pa. (24).
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Southworth, Mase S., Ph.D., Williamstown, Mass. (25).
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Springer, Alfred, Cincinnati, Ohio (24).
Stearns, Eben S., Chancellor Univ. of Nashville, Nashville, Tenn. (26).
Stearns, Winfrid Alden, Salem, Mass. (23).
Sternberg, George M., Surgeon U.S.A., Post Surgeon, Fort Walla Walla, W. T. (24).
Stevens, Thaddeus M., M.D., Indianapolis, Ind. (20).
Stewart, Prof. Elliott W., Lake View, Erie Co., N. Y. (25).
Stimpson, Thomas M., Peabody, Mass. (18).
St. John, Jas. A., St. Louis, Mo. (27).
Stone, Miss Mary H., Salem, Mass. (25).

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Storer, Prof. Frank H., Jamaica Plain, Mass. (13).
Stowell, John, 48 Main Street, Charlestown, Mass. (21).
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Townshend, Prof. N. S., Columbus, Ohio (17).
Tracy, Sam'l M., Columbia, Boone Co., Mo. (27).
Treat, Hon Sam'l, St. Louis, Mo. (27).
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Vail, Prof. Hugh D., 1927 Mt. Vernon St., Philadelphia, Pa. (18).



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Warring, Charles B., Croton Landing, N. Y. (25).
Watkins, Samuel, President Board of Education, Nashville, Tenn. (26).
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Weakley, Richard W., Supt. Public Schools, Nashville, Tenn. (26).
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Wheeler, Lt. Geo. M., U.S.E., Lock Box 93, Washington, D. C. (25).
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White, Lieut. Com'd H. C., U. S. Navy, U. S. S. Minnesota, Navy yard, New York (23).
White, Prof. I. C., University of W. Va., Morgantown, W. Va. (25).
White, John, Buffalo, N. Y. (25).
White, LeRoy S., Box 157, Waterbury, Conn. (23).
White, Samuel S., Publisher Dental Cosmos, Chestnut St., cor. Twelfth, Philadelphia, Pa. (23).
White, Z. L., Washington, D. C. (26).
Whitman, Chas. O., Newton Highlands, Mass. (27).
Whitney, Mary W., Waltham, Mass. (19).
Whitworth, John L., Nashville, Tenn. (26).
Wilder, Gen. John T., Chattanooga, Tenn. (26).
Wilkin Daniel F., Nashville Tenn. (26).
Williams, Charles H., Buffalo, N. Y. (25).
Williams, E. B., Strawberry Point, Iowa (24).
Williams, Edward H., jr., Drawer G., Wilkes Barre, Pa. (25).

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Williams, Willoughby, Nashville, Tenn. (26).
Williamson, Lieut. Col. R. S., U. S. Engineers, Light House Engineer, San Francisco, Cal. (12).
Wills, A. W., corner Union and Cherry streets, Nashville, Tenn. (26).
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Wilson, M. C., Russellville, Ala. (26).
Wilson, Walter T., Buffalo, N. Y. (25).
Wright, Prof. Albert A., Oberlin College, Oberlin, Ohio (24).
Wright, Rev. William J., Carlisle, Pa. (25).

Yeatman, Prof. Joseph W., Nashville, Tenn. (26).

Zalinski, Lieut. E. L., U. S. A., St. Augustine, Fla. (25).

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Abbe, Prof. Cleveland, Washington, D. C. (16). 1874.
Agassiz, Alex., Curator Mus. Comp. Zool., Cambridge, Mass. (18). 1874.
Alexander, John S., 1935 Arch St., Philadelphia, Pa. (20). 1874.
Allen, Zachariah, Providence, R. I. (1). 1874.
Alvord, Benj., Pay'r Gen., U. S. Army, Washington, D. C. (17). 1874.
Andrews, Prof. E. B., Lancaster, Ohio (7). 1876.
Appleton, Prof. John H., Brown Univ., Providence, R. I. (22). 1874.
Austin, E. P., 80 E. Brookline St., Boston, Mass. (18). 1874.

Bailey, W. W., 31 Market Square, Providence, R. I. (18). 1874.
Baird, Prof. S. F., Sec'y Smith. Inst., Washington, D. C. (1). 1874.
Barker, Prof. G. F., Univ. of Penn., Philadelphia, Pa. (13). 1874.
Barnard, F. A. P., President Columbia College, New York (7). 1874.
Barnard, Gen. J. G., U. S. Army, Army Building, New York (14). 1874.
Bassett, Homer F., Waterbury, Conn. (23). 1874.
Batchelder, Dr. J. H., Salem, Mass. (18). 1874.
Batchelder, John M., 3 Divinity Avenue, Cambridge, Mass. (8). 1875.
Bell, Samuel N., Manchester, N. H. (7). 1874.
Bethune, Rev. C. J. S., Trinity Coll. School, Pt. Hope, Canada (18). 1875.
Blackie, Geo. S., M. D., Nashville, Tenn. (26). 1877.
Blake, Clarence J., M. D., Hotel Berkeley, Boston, Mass. (24). 1877.
Blake, Eli W., New Haven, Conn. (1). 1874.
Blake, Prof. Eli W., jr., Providence, R. I. (15). 1874.
Blake, Francis, jr., U. S. Coast Survey, Newton Lower Falls, Mass. (23). 1874.
Bolton, Dr. H. Carrington, Trinity Coll., Hartford, Conn. (17). 1874.
Boss, Prof. Lewis, Director Dudley Observ., Albany, N. Y. (26). 1878.
Bouvé, Thos. T., Pres't Boston Soc. Nat. Hist., Boston, Mass. (1). 1874.
Bowditch, Henry I., M.D., 113 Boylston St., Boston, Mass. (2). 1875.
Brewer, Prof. Wm. H., Box 55, New Haven, Conn. (20). 1874.
Brocklesby, Prof. John, Trinity College, Hartford, Conn. (23). 1874.
Brooks, Wm. K., Johns Hopkins University, Baltimore, Md. (23). 1874.
Bross, Hon. Wm., Chicago, Ill. (7). 1874.
Broun, Prof. W. LeRoy, Vanderbilt Univ., Nashville, Tenn. (25). 1877.
Brown, Robert, jr., Office Gas Light Co., Cincinnati, Ohio (11). 1874.
Brown, Mrs. Robert, jr., Cincinnati, Ohio (17). 1874.
Brush, Prof. George J., Yale College, New Haven, Conn. (4). 1874.
Burgess, Edward, Sec. Natural History Soc., Boston, Mass (22). 1877.
Burnham, S. W., 52 Vincennes Ave., Chicago, Ill. (25). 1877.

Caldwell, Prof. Geo. C., Cornell University, Ithaca, N. Y. (23). 1875.
Canby, Wm. M., 1101 Delaware Avenue, Wilmington, Del. (17). 1878.

[1] The date at the end of the line indicates the year when the member was made a Fellow.

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Carpenter, Lieut. W. L., U. S. A., Dunkirk, N. Y. (24). 1877.
Carr, Lucien, Peab. Mus. Arch. and Ethn., Cambridge, Mass. (25). 1877.
Carrier, Prof. Jos. C., Pres't St. Mary's Coll., Galveston, Tex. (20). 1874.
Chadbourne, Prof. P. A., Pres't Williams Coll., Williamstown, Mass. (10). 1875.
Chamberlin, T. C., Beloit College, Beloit, Wis. (21). 1877.
Chandler, Prof. C. F., School of Mines, Columbia Coll., N. Y. (19). 1874.
Chandler, Prof. W. H., The Lehigh Univ., Bethlehem, Pa. (19). 1874.
Chanute, O., care Erie Railway, New York (17). 1877.
Chesbrough, E. S., Chicago, Ill. (2). 1877.
Chickering, Prof. J. W., jr., Deaf Mute College, Washington, D. C. (22). 1877.
Clark, Prof. John E., Mathematics, Yale College, New Haven, Conn. (17). 1875.
Clarke, Prof. F. W., Cincinnati University, Cincinnati, O. (18). 1874.
Coffin, Prof. John H. C., U. S. Navy, Washington, D. C. (1). 1874.
Coffin, Prof. Selden J., Ph.D. Lafayette College, Easton, Pa. (22). 1874.
Collett, Prof. John, Chief Ind. Bureau of Statistics of Geology, Indianapolis, Ind. (17). 1874.
Comstock, Prof. M. L., Galesburg, Knox Co., Ill. (21). 1874.
Comstock, Theo. B., Ass't Prof. in charge of Geol. Dep't, Cornell University, Ithaca, N. Y. (24). 1877.
Cook, Prof. George H., New Brunswick, N. J. (4). 1875.
Cope, Prof. Edward D., 2100 Pine St., Philadelphia, Pa. (17). 1874.
Coues, Elliott, Surgeon U. S. Army, Smithsonian Institution, Washington, D. C. (25). 1876.
Cox, Prof. Edward T., Indianapolis, Ind. (19). 1874.
Craik, Robert, M.D., McGill University, Montreal, Canada (11). 1874.
Cramp, Rev. J. M., D.D., Wolfville, N. S. (11). 1874.
Crocker, Charles F., Lawrence, Mass. (22). 1874.
Crocker, Susan E., M.D., Lawrence, Mass. (21.) 1874.
Cummings, Rev. Joseph, D.D., Malden, Mass. (13). 1874.
Curtis, Dr. Josiah, Washington, D. C. (18). 1874.
Cutting, Hiram A., M.D., State Geologist, Lunenburgh, Vt. (17).) 1874.

Dall, Mrs. Caroline H., Georgetown P. O., Washington, D. C. (18). 1874.
Dall, Wm. H., Smithsonian Institution, Washington, D. C. (18). 1874.
Dalrymple, Rev. Dr. E. A., Baltimore, Md. (11). 1874.
Dana, Edward Salisbury, New Haven, Conn. (23). 1874.
Dana, Prof. James D., New Haven, Conn. (1). 1874.
Danforth, Edward, Dep't of Public Instruction, Elmira, N. Y. (11). 1874.
Dawson, Dr. J. W., Principal McGill Coll., Montreal, Canada (10). 1874.
Day, F. H., M.D., Wauwatosa, Wis. (20). 1874.
Dean, George W., U. S. Coast Survey, Fall River, Mass. (15). 1874.
Dimmock, George, 19 Baldwin St., Cambridge, Mass. (22). 1874.
Dinwiddie, Robert, 113 Water St., New York (1). 1874.

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Dwyer, John, M. D., Hartford, Conn. (23). 1874.

Eaton, Prof. D. G., Packer Institute, Brooklyn, N. Y. (19). 1874.
Eddy, Prof. H. T., University of Cincinnati, Cincinnati, O. (24). 1875.
Edison, Thos. A., Menlo Park, N. J. (27). 1878.
Edwards, Thomas Carne, Cumberland Co., Vineland, N. J. (21). 1874.
Eimbeck, Wm., U.S.C.S., Washington, D. C. (17). 1874.
Elliott, Ezekiel B., Statistical Bureau, Washington, D. C. (10). 1874.
Elsberg, Louis, M.D., 614 Fifth Avenue, New York (23). 1874.
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Emerson, Prof. C. F., Dartmouth College, Hanover, N. H. (22). 1874.
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Emerton, James H., Salem, Mass. (18). 1875.
Engelmann, Dr. George, St. Louis, Mo. (1). 1874.
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Eustis, Prof. Henry L., Cambridge, Mass. (2). 1874.
Evans, Asher B., Principal Union School, Lockport, N. Y. (19). 1874.

Fairbanks, Rev. Henry, St. Johnsbury, Vt. (14). 1874.
Farlow, Dr. W. G., 6 Park Square, Boston, Mass. (20). 1874.
Farmer, Moses G., Torpedo Station, Newport, R. I. (9). 1875.
Ferrel, Wm., U. S. Coast Survey Office, Washington, D. C. (11). 1874.
Ficklin, Prof. Joseph, Univ. of Missouri, Columbia, Mo. (20). 1878.
Fitch, Edward H., Ashtabula, Ohio (11). 1874.
Fitch, O. H., Ashtabula, Ohio (7). 1874.
Fontaine, Prof. W. M., Univ. of W. Va., Morgantown, W. Va. (25). 1877.
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Genth, Prof. F. A., Univ. of Pennsylvania, Philadelphia, Pa. (24). 1875.
Gilbert, G. K., P. O. Box 806, Washington, D. C. (18). 1874.
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Gilman, Daniel C., President of the Johns Hopkins University, Baltimore, Md. (10). 1875.
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Goessman, Prof. C. A., Mass. Agric. Coll., Amherst, Mass. (18). 1875.
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Hall, Prof. James, Albany, N. Y. (1). 1874.
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Holden, Prof. E. S., Naval Observatory, Washington, D. C. (23). 1874.
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Hosford, Charles E., Terre Haute, Ind. (20). 1874.
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Hough, G. W., Chicago, Ill. (15). 1874.
Hoy, Philo R., M.D., Racine, Wis. (17). 1875.
Hunt, George, Providence, R. I. (9). 1874.
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Kerr, Prof. W. C., Raleigh, N. C. (10). 1874.
Kirkwood, Prof. Daniel, Bloomington, Ind. (7). 1874.

Lambert, Rev. Thomas R., D.D., Charlestown, Mass. (18). 1874.
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Lintner, J. A., N. Y. State Mus. of Nat. Hist., Albany, N. Y. (22). 1874.
Little, Prof. Geo., State Geologist of Georgia, Atlanta, Ga. (15). 1874.
Little, W. C., Albany, N. Y. (22). 1874.
Lockwood, Rev. Samuel, Freehold, Monmouth Co., N. J. (18). 1875.
Loomis, Prof. Elias, New Haven, Conn. (1). 1874.
Loughridge, Prof. R. H., Ass't State Geologist, Atlanta, Ga. (21). 1874.
Lovering, Prof. Joseph, Harvard University, Cambridge, Mass. (2). 1874.
Lupton, Prof. N. T., Vanderbilt University, Nashville, Tenn. (17). 1874.
Lyman, Prof. Chester S., New Haven, Conn. (4). 1874.
Lyman, Col. Theodore, Brookline, Mass. (23). 1874.
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Marcy, Prof. Oliver, Evanston, Ill. (10). 1874.
Marsh, Prof. O. C., Yale College, New Haven, Conn. (15). 1874.
Mason, Prof. Otis T., Columbian University, Washington, D. C. (25). 1877.
Mayer, Prof. A. M., Stevens Inst. Tech., Hoboken, N. J. (19). 1874.
McClintock, Frank, West Union, Iowa (22). 1874.
McMurtrie, William, Dep't Agriculture, Washington, D. C. (22). 1874.
McRae, Hamilton S., Sup't of Schools, Muncie, Ind. (20). 1874.
Means, Rev. A., Oxford, Ga. (5). 1877.
Meehan, Thomas, Germantown, Pa. (17). 1874.

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Mees, Carl Leo, Louisville, Ky. (24). 1876.
Meigs, Dr. James Aitken, 1408 Spruce St., Philadelphia, Pa. (12). 1875.
Mendenhall, Prof. T. C., Imperial University, Tokio, Japan, (20). 1874.
Mitchell, Miss Maria, Vassar College, Poughkeepsie, N. Y. (4). 1874.
Moore, Prof. J. W., M. D., Lafayette College, Easton, Pa. (22). 1874.
Morgan, Hon. L. H., Rochester, N. Y. (10). 1874.
Morley, Prof. Edward W., Hudson, Ohio (18). 1876.
Morris, Rev. John G., Baltimore, Md. (12). 1874.
Morse, Prof. E. S., Imperial Univ. of Tokio, Tokio, Japan. (18). 1874.
Morton, H., Stevens Institute Technology, Hoboken, N. J. (18). 1874.
Mudge, Benjamin F., Manhattan, Riley Co., Kansas (25). 1878.
Munroe, Prof. C. E., U. S. Naval Academy, Annapolis, Md. (22). 1874.

Nason, Prof. H. B., Rensselaer Polytechnic Inst., Troy, N. Y. (13). 1874.
Nelson, Prof. Edward T., Delaware, Delaware Co., O. (24). 1877.
Newberry, Prof. J. S., Cleveland, O., & Columbia Coll., N. Y. (5). 1874.
Newcomb, Prof. S., U. S. Naval Observ., Washington, D. C. (13). 1874.
Newton, Hubert A., New Haven, Conn. (6). 1874.
Nichols, Prof. W. R., Mass. Inst. Technology, Boston, Mass. (18). 1875.
Niles, Prof. W. H., Cambridge, Mass. (16). 1874.
Nipher, Prof. F. E., Washington University, St. Louis, Mo. (24). 1876.
Norton, Prof. W. A., New Haven, Conn. (6). 1874.

Oliver, Prof. James E., Cornell University, Ithaca, N. Y. (7). 1874.
Oliver, Miss Mary E., Cascadilla Hotel, Ithaca, N. Y. (20). 1874.
Orton, Prof. Edward, President Ohio Agricultural and Mechanical College, Columbus, Ohio (19). 1875.
Osborne, J. W., 212 Delaware Ave., N. E., Washington, D. C. (22). 1874.
Owen, Dr. R., Indiana State University, Bloomington, Ind. (20). 1874.

Paine, Cyrus F., Rochester, N. Y. (12). 1874.
Paine, Nathaniel, Worcester, Mass. (18). 1874.
Palfray, Hon. Charles W., Salem, Mass. (21). 1874.
Parkhurst, H. M., Law Stenographer, 33 Park Row, New York (23). 1874.
Parry, Dr. Charles C., Davenport, Iowa (6). 1875.
Peckham, S. F., Univ. of Minnesota, Minneapolis, Minn. (18). 1875.
Pedrick, Wm. R., Lawrence, Mass. (22). 1874.
Peirce, B. O., 413 Broadway, Cambridge, Mass. (18). 1874.
Perkins, Maurice, Schenectady, N. Y. (15). 1874.
Pettee, Prof. William H., Ann Arbor, Mich. (24). 1875.
Phippen, Geo. D., Salem, Mass. (18). 1874.
Pickering, Prof. E. C., Director of Observ., Cambridge, Mass. (18). 1874.
Pourtales, L. F., Keeper Mus. Comp. Zool., Cambridge, Mass. (1). 1874.
Powell, Major J. W., 910 M St., Washington, D. C. (23). 1875.

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Prescott, Prof. Albert B., Ann Arbor, Mich. (23). 1874.
Prime, Frederick, jr., Ass't Geologist of Pa., 907 Walnut St., Philadelphia, Pa. (24). 1874.
Pumpelly, Prof. Raphael, Owego, Tioga County, N. Y. (17). 1874.
Putnam, F. W., Curator Peabody Museum Archæology and Ethnology, Cambridge, Mass. Address as Permanent Secretary A. A. A. S., Salem, Mass. (10). 1874.
Pynchon, Rev. T. R., Pres't of Trinity Coll., Hartford, Conn. (23). 1875.

Quimby, Prof. E. T., Hanover, N. H. (22). 1874.
Quincy, Edmund, 85 Clinton St., Boston, Mass. (11). 1874.

Rauch, Dr. John H., Chicago, Ill. (11). 1875.
Raymond, Rossiter W., 17 Burling Slip, New York (15). 1875.
Redfield, J. H., care A. Whitney and Sons, Philadelphia, Pa. (1). 1874.
Rees, Prof. John K., Washington University, St. Louis, Mo. (26). 1878.
Remsen, Prof. Ira, 11 Cathedral St., Baltimore, Md. (22). 1875.
Rice, Prof. Wm. North, Middletown, Conn. (18). 1874.
Richards, Prof. Robert H., Mass. Inst. Tech., Boston, Mass. (22). 1875.
Richards, Mrs. R. H., Mass. Inst. of Tech., Boston, Mass. (23). 1878.
Riley, Prof. C. V., U. S. Entomologist, Washington, D. C. (17). 1874.
Ritchie, E. S., 150 Tremont St., Boston, Mass. (10). 1877.
Rockwood, Prof. Charles G., jr., Princeton, N. J. (20). 1874.
Rogers, F., 202 West Rittenhouse Square, Philadelphia, Pa. (11). 1874.
Rogers, Prof. Wm. B., 117 Marlborough St, Boston, Mass. (1). 1874.
Rood, Prof. O. N., New York (14). 1874.
Roosevelt, Clinton, 11 Wall St., New York (11). 1874.
Runkle, Prof. J. D., Mass. Inst. of Tech., Boston, Mass. (2). 1874.
Rutherford, Lewis M., 175 Second Ave., New York (13). 1875.

Sadtler, Prof. S. P., University of Penn., Philadelphia, Pa. (22). 1875.
Safford, Dr. James M., Nashville, Tenn. (6). 1874.
Saunders, William, London, Ontario, Canada (17). 1874.
Schott, Charles A., Coast Survey Office, Washington, D. C. (8). 1874.
Schweitzer, Prof. Paul, State Univ. of Mo., Columbia, Mo. (24). 1877.
Scudder, Samuel H., Cambridge, Mass. (13). 1874.
Seaman, W. H., Microscopist, 1424 11th St. N. W. Washington, D. C. (23). 1874.
Shaler, Prof. N. S., Harvard University, Cambridge, Mass. (19). 1874.
Sias, Solomon, M.D., Schoharie, Schoharie Co., N. Y. (10). 1874.
Sill, Hon. Elisha N., Cuyahoga Falls, Ohio (6). 1874.
Silliman, Prof. Benj., Yale College, New Haven, Conn. (1). 1874.
Silliman, Prof. Justus M., Lafayette College, Easton, Pa. (19). 1874.
Smith, Prof. E. A., University of Alabama, Tuscaloosa, Ala. (20). 1877.
Smith, Dr. J. Lawrence, Louisville, Ky. (1). 1874.

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Smith, Prof. S. I., Yale College, New Haven, Conn. (18). 1874.
Spencer, J. W., Teacher & Geol., Paxton, Sullivan Co., Ind. (20). 1874.
Stanard, Benjamin A., Cleveland, Ohio (6). 1874.
Stearns, Henry P., M.D., Hartford, Conn. (23). 1874.
Stearns, R. E. C., P. O. Box 13, Berkeley, Cal. (18). 1874.
Steiner, Dr. Lewis H., Frederick City, Md. (7). 1874.
Stephens, W. Hudson, Lowville, N. Y. (18). 1874.
Stockwell, John N., 579 Case Avenue, Cleveland, Ohio (18). 1874.
Stone, Mrs. Leander, 1571 Indiana Avenue, Chicago, Ill. (22). 1874.
Stone, Ormond, Director Cincinnati Observatory, Univ. of Cincinnati, Mt. Lookout, Ohio (24). 1876.
Storrs, Henry E., Jacksonville, Ill. (20). 1874.
Stuart, Prof. A. P. S., Lincoln, Nebraska. (21). 1874.
Stubbs, Prof. William C., Alabama Agricultural and Mechanical College, Auburn, Lee Co., Ala. (25). 1877.

Tappan, Prof. Eli T., Kenyon College, Gambier, Ohio (20). 1874.
Terry, Prof. N. M., U. S. Naval Academy, Annapolis, Md. (23). 1874.
Thompson, A. Remsen, 50 West 51st Street, New York (1). 1874.
Thurston, Prof. R. H., Stevens Institute, Hoboken, N. J. (23). 1874.
Toner, J. M., M.D., 615 Louisiana Ave., Washington, D. C. (23). 1875.
Trowbridge, Prof. John, Harvard University, Cambridge, Mass. (25.) 1876.
Trowbridge, Prof. W. P., New Haven, Conn. (10). 1874.
Turnbull, Laurence, M.D., 1208 Spruce St., Philadelphia, Pa. (10). 1874.
Tuttle, Prof. Albert H., Columbus, Ohio (17). 1874.

Uhler, Philip R., Baltimore, Md. (19). 1874.

Van der Weyde, Dr. P. H., P. O. Box 4379, New York (17). 1874.
Vaux, William S., 1702 Arch St., Philadelphia, Pa. (1). 1874.
Vose, Prof. Geo. L., Bowdoin College, Brunswick, Me. (15). 1875.

Wadsworth, M. Edward, Assistant in Lithology, Museum of Comparative Zoology, Cambridge, Mass. (23). 1874.
Walker, Prof. J. R., Napoleon Ave., cor. Coliseum St., New Orleans, La. (19). 1874.
Waller, E., School of Mines, Columbia College, New York (23). 1874.
Walling, H. F., 102 Chauncy St., Boston, Mass. (16). 1874.
Ward, Prof. Henry A., Rochester, N. Y. (13). 1875.
Ward, Henry D. A., Meteorologist, Middletown, Conn. (23). 1874.
Ward, Dr. R. H., 53 Fourth St., Troy, N. Y. (17). 1874.
Warner, James D., 199 Baltic St., Brooklyn, N. Y. (18). 1874.
Warren, Gen. G. K., U.S.A., Engineer's Office, Newport, R. I. (12). 1874.



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Warren, Prof. S. Edward, Newton, Mass. (17). 1874.
Watson, Sereno, Botanic Gardens, Cambridge, Mass. (22). 1874.
Webster, Prof. N. B., Principal Webster Inst., Norfolk, Va. (7). 1874.
Wells, Daniel H., Hartford, Conn. (18). 1874.
Westcott, O. S., Racine, Wis. (21). 1874.
Wetherby, Albert G., Univ. of Cincinnati, Cincinnati, Ohio (26). 1877.
Wheatland, Dr. Henry, President Essex Inst., Salem, Mass. (1). 1874.
Wheildon, W. W., Concord, Mass. (13). 1874.
White, Prof. C. A., Le Droit Park, Washington, D. C. (17). 1874.
Whitfield, R. P., 173 Elm St., Albany, N. Y. (18). 1874.
Whitney, Solon F., Watertown, Mass. (20). 1874.
Whittlesey, Col. Charles, Cleveland, Ohio (1). 1874.
Wilber, G. M., Pine Plains, N. Y. (19). 1874.
Wilbur, A. B., Port Jervis, Orange Co., N. Y. (23). 1874.
Wilder, Prof. Burt G., Cornell University, Ithaca, N. Y. (22). 1874.
Wiley, Prof. Harvey W., Purdue Univ., La Fayette, Ind. (21). 1874.
Willet, Prof. Joseph E., Mercer Univ., Macon, Ga. (25). 1878.
Williams, Charles H., M.D., 15 Arlington St., Boston, Mass. (22). 1874.
Williams, Prof. Henry W., 15 Arlington St., Boston, Mass. (11). 1874.
Wilson, Prof. Daniel, 117 Bloor St., Toronto, Canada (25.) 1876.
Winchell, Prof. Alex., Ann Arbor, Mich. (3). 1874.
Winchell, Prof. N. H., Univ. of Minnesota, Minneapolis, Minn. (19). 1874.
Witter, F. M., Muscatine, Iowa (21). 1874.
Wormley, T. G., Univ. of Pennsylvania, Philadelphia, Pa. (20). 1878.
Worthen, A. H., Springfield, Ill. (5). 1874.
Wright, Prof. Arthur W., Yale College, New Haven, Conn. (14). 1874.
Würtele, Rev. Louis C., Acton Vale, Province of Quebec, C. E. (11). 1874.
Wyckoff, Wm. C., 44 Howard St., New York (20). 1874.
Wylie, Prof. Theoph. A., Indiana Univ., Bloomington, Ind. (20). 1874.

Yarrow, Dr. H. C., care Army Med. Mus., Washington, D. C. (23). 1874.
Youmans, Prof. Edward L., New York (6). 1874.
Young, C. A., Prof. of Astronomy, College of New Jersey, Princeton, New Jersey (18.) 1874.

[344 FELLOWS.]

TOTAL NUMBER OF MEMBERS OF THE ASSOCIATION, 962.

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# DECEASED MEMBERS.

[Information respecting omissions in this list, and the date of birth and of decease of any of the former members, is requested by the Permanent Secretary.]

Abert, J. J., Washington, D. C. (1). Died in 1862.
Adams, C. B., Amherst, Mass. (1).
Adams, Edwin F., Charlestown, Mass. (18).
Adams, Samuel, Jacksonville, Ill. (18). Born Dec. 19, 1806. Died April 29, 1877.
Agassiz, Louis, Cambridge, Mass. (1). Born May 28, 1807. Died Dec. 14, 1873.
Ainsworth, J. G., Barry, Mass. (14).
Allston, R. F. W., Georgetown, S. C. (3). Died in 1864.
Anthony, Charles H., Albany, N. Y. (6). Died in 1874.
Ames, N. P., Springfield, Mass. (1).
Appleton, Nathan, Boston, Mass. (1).
Armstrong, John W., Fredonia, N. Y. (24).
Atwater, Mrs. S. T., Chicago, Ill. (17). Born Aug. 8, 1812. Died in April, 1878.
Bache, Alexander D., Washington, D. C. (1). Born July 19, 1806. Died Feb. 17, 1867.
Bache, Franklin, Philadelphia, Pa. (1).
Bailey, J. W., West Point, N. Y. (1).
Bardwell, F. W., Lawrence, Kan. (13.) Died in 1878.
Barrett, Moses, Milwaukee, Wis. (21). Died in 1873.
Beck, C. F., Philadelphia, Pa. (1).
Beck, Lewis C., New Brunswick, N. J. (1).
Beck, T. Romeyn, Albany, N. Y. (1).
Belt, Thomas, London, Eng. (27). Died Sept 8, 1878.
Benedict, G. W., Burlington, Vt. (16).
Bicknell, Edwin, Boston, Mass. (18). Born in 1830. Died March 19, 1877.
Binney, Amos, Boston, Mass. (1).
Binney, John, Boston, Mass. (3).
Blanding, William, ——, R. I. (1).
Blatchford, Thomas W., Troy, N. Y. (6).
Blatchley, Miss S. L., New Haven, Conn. (19).
Boadle, John, Haddonfield, N. J. (20). Born in 1805. Died in July, 1878.
Bomford, George, Washington, D. C. (1).
Bowron, James, South Pittsburg, Tenn. (26). Died in Dec., 1877.
Bradley, Leverette, Jersey City, N. J. (15). Died in 1875.
Braithwaite, Jos., Chambly, C. W. (11).
Brigham, Charles H., Ann Arbor, Mich. (17). Born in 1820. Died in Jan., 1879.
Brown, Andrew, Natchez, Miss. (1).
Burnap, G. W., Baltimore, Md. (12).
Burnett, Waldo I., Boston, Mass. (1).
Butler, Thomas B., Norwalk, Conn. (10).
Carpenter, Thornton, Camden, S. C. (7).
Carpenter, William M., New Orleans, La. (1).
Case, William, Cleveland, Ohio (6).
Caswell, Alexis, Providence, R. I. (2). Died Jan. 8, 1877.
Chapman, N., Philadelphia, Pa. (1).
Chase, S., Dartmouth, N. H. (2).



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Chauvenet, William, St. Louis, Mo. (1).
Clapp, Asahel, New Albany, Ind. (1).
Clark, H. J., Cambridge, Mass. (13).
Clark, Joseph, Cincinnati, Ohio (5).
Clarke, A. B., Holyoke, Mass. (13).
Cleaveland, C. H., Cincinnati, Ohio (9).
Cleveland, A. B., Cambridge, Mass. (2).
Coffin, James H., Easton, Pa. (1).
Cole, Thomas, Salem, Mass. (1). Born Dec. 24, 1779. Died June 24, 1852.
Coleman, Henry, Boston, Mass. (1).
Conrad, Timothy Abbott, Philadelphia, Pa. (1). Born in August, 1803. Died August 9, 1877.
Cooper, William, Hoboken, N. J. (9). Died in 1864.
Corning, Erastus, Albany, N. Y. (6).
Couper, J. Hamilton, Darien, Ga. (1).
Crosby, Alpheus, Salem, Mass. (10). Born Oct. 13, 1810. Died Apr. 17, 1874.
Crosby, Thomas R., Hanover, N. H. (18).
Croswell, Edwin, Albany, N. Y. (6).
Curry, W. F., Geneva, N. Y. (11).
Dayton, Edwin A., Madrid, N. Y. (7).
Dean, Amos, Albany, N. Y. (6).
Dearborn, George H. A. S., Roxbury, Mass. (1).
Dekay, James E., New York (1).
DeLaski, John, Carver's Harbor, Me. (18).
Dewey, Chester, Rochester, N. Y. (1).
Dexter, G. M., Boston, Mass. (11).
Dillingham, W. A. P., Augusta, Me. (17).
Doggett, Wm. E., Chicago, Ill. (17). Born Nov. 20, 1820. Died in 1876.
Doolittle, L., Lenoxville, C. E. (11). Died in 1862.
Ducatel, J. T., Baltimore, Md. (1).
Duffield, George, Detroit, Mich. (10). Died in 1868.
Dumont, A. H., Newport R. I. (14).
Duncan, Lucius C., New Orleans, La. (10).
Dunn, R. P., Providence, R. I. (14).
Easton, Norman, Fall River, Mass. (14).
Eaton, James H., Beloit, Wis. (17). Died Jan. 5, 1877.
Ely, Charles Arthur, Elyria, Ohio (4).
Emmons, Ebenezer, Williamstown, Mass. (1).
Engstrom, A. B., Burlington, N. J. (1).
Everett, Edward, Boston, Mass. (2).
Ewing, Thomas, Lancaster, Ohio (5).
Faries, R. J., Wauwatosa, Wis. (21). Died May 31, 1878.
Ferris, Isaac, New York (6).
Feuchtwanger, Lewis, New York (11). Died in 1876.
Fillmore, Millard, Buffalo, N. Y. (7).
Fisher, Mark, Trenton, N. J. (10).
Fitch, Alexander, Hartford, Conn. (1).
Forbush, E B., Buffalo, N. Y. (15).
Force, Peter, Washington, D. C. (4.)
Ford, A. C , Nashville, Tenn. (26).
Foster, J. W., Chicago, Ill. (1). Died June 29, 1873.
Foucon, Felix, Madison, Wis. (18).
Fowle, Wm. B., Boston, Mass. (1). Died in 1864.
Fox, Charles, Grosse Ile, Mich. (7).
Frazer, John F., Philadelphia, Pa. (1).
French, J. W., West Point, N. Y. (11).
Gavit, John E., New York (1).
Gay, Martin, Boston, Mass. (1).

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Gibbon, J. H., Charlotte, N. C. (3).
Gillespie, W. M., Schenectady, N. Y. (10.)
Gilmor, Robert, Baltimore, Md. (1).
Gould, Augustus A., Boston, Mass. (11). Died Sept. 15, 1866.
Gould, B. A., Boston, Mass. (2).
Graham, James D., Washington, D. C. (1).
Gray, Alonzo, Brooklyn, N. Y. (13).
Gray, James H., Springfield, Mass. (6).
Greene, Benjamin D , Boston, Mass. (1).
Greene, Everett W., Madison, N. J. (10). Died in 1864.
Greene, Samuel, Woonsocket, R. I. (9). Died in 1868.
Greer, James, Dayton, Ohio (20).
Griffith, Robert E., Philadelphia, Pa. (1).
Griswold, John A., Troy, N. Y. (19).
Guest, William E., Ogdensburg, N. Y. (6).
Hackley, Charles W., New York (4).
Hale, Enoch, Boston, Mass. (1).
Hance, Ebenezer, Fallsington P. O., Pa. (7). Died in 1876.
Hare, Robert, Philadelphia, Pa. (11).
Harlan, Joseph G., Haverford, Pa. (8).
Harlan, Richard, Philadelphia, Pa. (1).
Harris, Thaddeus W., Cambridge, Mass. (1). Born Nov. 12, 1795. Died Jan. 16, 1856.
Harrison, Jos., jr., Philadelphia, Pa. (12).
Hart, Simeon, Farmington, Conn. (1).
Hartt, Charles F., Ithaca N. Y. (18). Born in 1840. Died March 18, 1878.
Haven, Joseph, Chicago, Ill. (17). Died in 1874.
Hayden, H. H., Baltimore, Md. (1).
Hayward, James, Boston, Mass. (1).
Henry, Jos., Washington, D. C. (1). Born Dec. 17, 1797. Died May 13, 1878.
Hickox, S. V. R., Chicago, Ill. (17). Died in 1872.
Hilgard, Theo. C., St. Louis, Mo. (17).
Hincks, William, Toronto, C. W. (11).
Hitchcock, Edward, Amherst, Mass. (1).
Hodgson, W. B., Savannah, Ga. (10).
Holbrook, J. E., Charleston, S. C. (1).
Hopkins, Albert, Williamstown, Mass. (19).
Hopkins, James G., Ogdensburg, N. Y. (10). Died in 1860.
Hopkins, T. O., Williamsville, N. Y. (10). Died in 1866.
Hosford, Benj. F., Haverhill, Mass. (13). Died in 1864.
Horton, C. V. R., Chaumont, N. Y. (10). Died in 1862.
Horton, William, Craigville, N. Y. (1).
Houghton, Douglas, Detroit, Mich. (1).
Hovey, Edmund O , Crawfordsville, Ind. (20). Born July 15, 1801. Died March 10, 1877.
Howland, Theodore, Buffalo, N. Y. (15).
Hubbert, James, Richmond, Province of Quebec (16). Died in 1868.
Hunt, E. B., Washington, D. C. (2).
Hunt, Freeman, New York (11).
Ives, Moses B , Providence, R. I. (9). Died in 1857.
Ives, Thomas P., Providence, R. I. (10).
Johnson, W. R., Washington, D. C. (1).
Jones, Catesby A. R., Washington, D. C. (8).
Keely, G. W., Waterville, Me. (1).
Keep, N. C., Boston, Mass. (13). Died in 1875.
Kennicott, Robert, West Northfield, Ill. (12).
King, Mitchell, Charleston, S. C. (3). Died in 1862.
Knickerbocker, Charles, Chicago, Ill. (17). Died in 1873.
Lacklan, R., Cincinnati, Ohio (11).

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Lapham, Increase A., Milwaukee, Wis. (3). Born Mch. 7, 1811. Died Sept. 14, 1875.
LaRoche, R., Philadelphia, Pa. (12).
Lasel, Edward, Williamstown, Mass. (1).
Lawford, Frederick, Montreal, Canada (11). Died in 1866.
Lederer, Baron von, Washington, D. C. (1).
Lieber, Oscar M., Columbia, S. C. (8).
Lincklaen, Ledyard, Cazenovia, N. Y. (1).
Linsley, James H., Stafford, Conn. (1).
Lockwood, Moses B., Providence, R. I. (9). Died in 1872.
Logan, William E., Montreal, Canada (1).
Loosey, Charles F., New York (12).
Lothrop, Joshua R., Buffalo, N. Y. (15).
Lyon, Sidney S., Jeffersonville, Ind. (20).
Maack, G. A., Cambridge, Mass. (18).
Mahan, D. H., West Point, N. Y. (9). Died in 1871.
Marsh, Dexter, Greenfield, Mass. (1).
Marsh, James E., Roxbury, Mass. (10).
Mather, William W., Columbus, Ohio (1).
Maupin, S., Charlottesville, Va. (10).
McMahon, Mathew, Albany, N. Y. (11).
M'Conihe, Isaac, Troy, N. Y. (5).
Meade, George G., Philadelphia, Pa. (15).
Meek, F. B., Washington, D. C. (6). Born December 10, 1817. Died December 21, 1876.
Mitchell, O. M., Cincinnati, Ohio (3).
Mitchell, William, Poughkeepsie, N. Y. (3). Died in 1869.
Mitchell, Wm. H., Florence, Ala. (17).
Morton, S. G., Philadelphia, Pa. (1).
Munroe, Nathan, Bradford, Mass. (6). Died in 1866.
Munroe, William, Concord, Mass. (18). Died April 27, 1877.
Newton, E. H., Cambridge, N. Y. (1).
Nichols, C. A., Providence, R. I. (17). Died in 1877.
Nicollett, J. N., Washington, D. C. (1).
Norton, J. P., New Haven, Conn. (1).
Noyes, J. O., New Orleans, La. (21).
Nutt, Cyrus, Bloomington, Ind. (20). Died in 1875.
Oakes, Wm., Ipswich, Mass. (1). Born July 1, 1799. Died July 31, 1848.
Ogden, Robert W., New Orleans, La. (21). Died March 24, 1878.
Ogden, William B., High Bridge, Westchester Co., N. Y. (17). Born in 1805. Died Aug. 3, 1877.
Olmsted, Alexander F., New Haven, Conn. (4).
Olmsted, Denison, New Haven, Conn. (1).
Olmsted, Denison, jr., New Haven, Conn. (1).
Painter, Jacob, Lima, Pa. (23). Died in 1876.
Painter, Minshall, Lima, Pa. (7).
Parker, Wilbur F., West Meriden, Conn. (23). Died in 1876.
Parkman, Samuel, Boston, Mass. (1).
Perkins, George R., Utica, N. Y. (1). Died in 1876.
Perkins, Henry C., Newburyport, Mass. (18). Born Nov, 13, 1804. Died Feb. 2, 1873.
Perry, John B., Cambridge, Mass. (16).
Perry, M. C., New York (10).
Piggot, A. Snowden, Baltimore, Md. (10).
Plumb, Ovid, Salisbury, Conn. (9).
Pope, Charles A., St. Louis, Mo. (12).
Porter, John A., New Haven, Conn. (14).
Pruyn, John V. L., Albany, N. Y. (1). Born in 1811. Died in 1877.
Pugh, Evan, Centre Co., Penn. (14).

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Putnam, Mrs. F. W., Cambridge, Mass. (19). Born Dec. 29, 1838. Died March 10, 1879.
Read, Ezra, Terra Haute, Ind. (20). Died in 1877.
Redfield, William C., New York (1).
Resor, Jacob, Cincinnati, Ohio (8). Died in 1871.
Robb, James, Fredericton, N. B. (4).
Robinson, Coleman T., Buffalo, N. Y. (15).
Rockwell, John A., Norwich, Conn. (10).
Rogers, Henry D., Glasgow, Scotland (1).
Rogers, James B., Philadelphia, Pa. (1).
Sager, Abram, Ann Arbor, Mich. (6). Died in 1877.
Schaeffer, Geo. C., Washington, D. C. (1). Died in 1873.
Scott, Joseph, Dunham, C. E. (11). Died in 1865.
Senter, Harvey S., Aledo, Ill. (20). Died in 1875.
Seward, William H., Auburn, N. Y. (1).
Sherwin, Thomas, Dedham, Mass. (11). Died in 1869.
Silliman, Benjamin, New Haven, Conn. (1).
Skinner, John B., Buffalo, N. Y. (15). Died in 1871.
Slack, J. H., Philadelphia, Pa. (12).
Smith, James Y., Providence, R. I. (9). Died in 1876.
Smith, J. V., Cincinnati, Ohio (5).
Smith, Lyndon A., Newark, N. J. (9).
Snell, Ebenezer S., Amherst, Mass. (2). Died in 1877.
Sparks, Jared, Cambridge, Mass. (2).
Stimpson, William, Chicago, Ill. (12). Died May 26, 1872.
Stone, Samuel, Chicago, Ill. (17). Died in 1876.
Sullivant, W. S., Columbus, Ohio (7).
Swain, James, Fort Dodge, Iowa (21). Born in 1816. Died in 1877.
Tallmadge, James, New York (1).
Taylor, Richard C., Philadelphia, Pa. (1).
Tenney, Sanborn, Williamstown, Mass. (17). Born in January, 1827. Died July 11, 1877.
Teschemacher, J. E., Boston, Mass. (1).
Thompson, Alexander, Aurora, N. Y. (6).
Thompson, Z., Burlington, Vt. (1).
Thurber, Isaac, Providence, R. I. (9).
Tillman, Samuel D., Jersey City, N. J. (15). Died in 1875.
Tolderoy, James B., Fredericton, N. B. (11).
Torrey, John, New York (1).
Torrey, Joseph, Burlington, Vt. (2). Died Nov. 26, 1867.
Totten, J. G., Washington, D. C. (1).
Townsend, Howard, Albany, N. Y. (10). Died in 1867.
Townsend, John K., Philadelphia, Pa. (1).
Townsend, Robert, Albany, N. Y. (9). Died in 1866.
Troost, Gerard, Nashville, Tenn. (1).
Tuomey, M., Tuscaloosa, Ala. (1).
Tyler, Edward R., New Haven, Conn. (1).
Vancleve, John W., Dayton, Ohio (1).
Vanuxem, Lardner, Bristol, Pa. (1).
Wadsworth, James S., Genesee, N. Y. (2).
Wagner, Tobias, Philadelphia, Pa. (9).
Walker, Joseph, Oxford, N. Y. (10).
Walker, Sears C., Washington, D. C. (1).
Walker, Timothy, Cincinnati, Ohio (4).
Walsh, Benjamin D., Rock Island, Ill. (17).
Wanzer, Ira, Brookfield, Conn. (18). Died March 5, 1879.
Warren, John C., Boston, Mass. (1). Born Aug. 1, 1778. Died May 4, 1856.
Watertown, Charles, Wakefield, Eng. (1).

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Webster, H. B., Albany, N. Y. (1).
Webster, J. W., Cambridge, Mass. (1).
Webster, M. H., Albany, N. Y. (1).
Weed, Monroe, Wyoming, N. Y. (6). Died in 1867.
Weyman, G. W., Pittsburg, Pa. (6). Died in 1864.
Wheatland, Richard H., Salem, Mass. (13).
Whitman, Wm. E , Philadelphia, Pa. (23). Died in 1875.
Whitney, Asa, Philadelphia, Pa. (1).
Whittlesey, Charles C., St. Louis, Mo. (11). Died in 1872.
Willard, Emma, Troy, N. Y. (15).
Wilson, W. C., Carlisle, Pa. (12).
Winlock, Joseph, Cambridge, Mass. (5). Died in 1875.
Woodbury, L., Portsmouth, N. H. (1).
Woodman, John S., Hanover, N. H. (11). Died in 1871.
Wright, John, Troy, N. Y. (1).
Wyman, Jeffries, Cambridge, Mass. (1). Born Aug. 11, 1814. Died Sept. 4, 1874.
Yarnall, M., Washington, D. C. (26). Born in 1817. Died Jan. 27, 1879.
Young, Ira, Hanover, N. H. (7).

[287 DECEASED MEMBERS.]

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# ADDRESS

OF

## PROFESSOR SIMON NEWCOMB,

THE RETIRING PRESIDENT OF THE ASSOCIATION.

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LADIES AND GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:—

IN imposing on its retiring President the duty of delivering an address, the constitution of this society sets no limits to his choice of a theme. Both in these and in the corresponding addresses delivered before the sister society of Great Britain, it has not been uncommon for the speaker to choose for his subject the general progress of scientific research during the year. This course is now less common than formerly, because, owing to the immensity of the field of research, it has become impossible for any ordinary mind to follow its progress in all its branches. I have thought, therefore, that a higher interest would attach to a theme chosen from the field of modern scientific thought, and, by a process in which I have been the follower rather than the leader of my own contemplations, I have been led to present to you some thoughts on the Course of Nature as seen in the light of modern scientific and philosophic research. Though I have but a single central idea to present to you, namely, that of the simplicity and universality of the Laws of Nature, yet so great is the confusion of thought which prevails on the question, What are the Laws of



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Nature, that it is necessary to approach my idea from more than one standpoint, and to illustrate it in more than one way.

We all know that the history of the Caucasian race, during the last three centuries, has been marked by a kind of intellectual development so entirely without precedent that some might call it miraculous; in fact, by such a development of the understanding of the course of nature as has revolutionized human society in many of its phases. You also know that this development has been marked by frequent collisions of opinion between the investigators of the material manifestations of nature on the one side (if I may be allowed to use the expression), and philosophers and theologians on the other, respecting the true theory of the course of nature. My desire in entering this field is to act the part of the peacemaker rather than that of a combatant, not sustaining any other propositions than those which are actually believed in by the large majority of educated men at the present time; but the confusion of thought on this subject, to which I have just alluded, is so great that, although I may combat no opinions actually held, it may be necessary to greatly modify their application, and to criticise the forms in which they have found expression.

The key-note of my discourse is found in a proposition which is fundamental in the history of modern science, and without a clear understanding of which everything I say may be entirely misunderstood. This proposition is, that science concerns itself only with phenomena and the relations which connect them, and does not take account of any questions which do not in some way admit of being brought to the test of observation. The only universe it knows is that made known by the telescope, the microscope, and other appliances of observation. That this is the whole universe we should all be very sorry to suppose, and none more so than he who has the honor to address you. But, should I pretend to a scientific knowledge of what lies behind this visible frame, I should be acting the part of the rash speculator rather than of the cautious thinker. Only into a single field of thought do I dare to venture. When we trace the efforts of men to penetrate the secrets of nature, we find them clearly divisible into two classes: philosophic speculation, and scientific investigation. We find the objects of thought equally divisible into two classes: phenomena and their hidden causes, those unknowable entities out of which

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they proceed. The great progress which the last three centuries have witnessed has been wholly in the field of phenomena, and it is to this field, and to the results of scientific investigation in it, to which I ask your attention this evening. But, it is to be expected that, in this brief characterization of our field of thought, I have failed to convey to your minds any clear conception of its boundaries. The progress here alluded to has been rendered possible only by entirely rejecting the mode of thinking about nature which was prevalent in former ages, and into which the untrained mind is almost sure to fall at the present day. The distinction will be evident to one mind at a glance, while another may be unable to comprehend it after all the explanations which it is possible to give. As my whole discourse will be misleading unless all my hearers have a clear conception of it, I shall endeavor to present you with the materials of such a conception, rather in the form of concrete illustrations in familiar language, than in that of abstract general definitions.

As one mode of expression, we might say that modern science introduces into the higher modes of thought about nature that same kind of practical good sense which characterizes the successful man of business. Scientific investigation is, in a certain sense, purely practical in both its methods and its aims. There is a mental operation, with which all are well acquainted, under the familiar term "theorizing;" to this operation all scientific investigation is so much opposed that the mere theorizer and essayist can never make any real advance in the knowledge of nature. To speak with a little more precision, we may say, that as science only deals with phenomenon and the laws which connect them, so all the terms which it uses have exact literal meanings, and refer only to things which admit of being perceived by the senses, or, at least, of being conceived as thus perceptible. This purely literal meaning of all scientific language is in strong contrast to the metaphorical and poetical forms of expression into which we are apt to fall in discourse upon abstract subjects generally, where our ideas cannot be at once referred to sensuous impressions.

We might also say, that no question is a scientific one which does not in some way admit of being tested by experience. The single object of scientific research is to predict the course of nature, or the results of those artificial combinations of causes which we call experiments; and no question is a scientific one

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unless its solution will in some way advance this object. I must not, however, be understood as saying that the test of experience can always be immediately applied, because then no disputed question could be a scientific one. For example, the question whether man existed on the earth 50,000 years ago is a scientific one, because it is one respecting actual historic occurrence of scenes evident to the senses. It could at once be settled by simple inspection, could we in any way form a picture of the earth as it then looked, and it may actually be settled in the future by the presence or absence of sensible traces of the existence of man at those times. Should we, however, go farther, and inquire whether such men had souls, our inquiry would not be a scientific one, nor one in which science could in any way concern itself with profit. The soul can neither be seen, nor in any way made evident to the senses of others. From the very nature of things, it could leave no material trace of itself to be unearthed by the geologist or antiquarian of a future age. So far are we from forming any conception even of our own souls, as sensible existences, that no question affecting them, even now, is a scientific one; much less can science consider those of past generations.

There is thus a quite well defined limit between questions which are scientific ones and those which are not scientific, and with which, in consequence, science has no concern whatever. You must not understand me as in any way claiming that questions of this last class are not worth thinking about. They include many which are of the most absorbing interest to the human race, and about which men will think the more as they become more thoughtful. But to mix them with scientific discussions will only introduce confusion of thought respecting sensible things, without in any manner advancing their solutions. The current desires that science shall consider man as something more than an animal are as unreasonable as if we wanted to make algebra a help to moral philosophy.

This limitation of all scientific research to a single specific field is something so little understood, that I may have occasion to call it to mind in other connections. But, there is another equally essential maxim of science which I must explain in order that you may understand the spirit which animates scientific investigation. It is, that the man of science, as such, has no preconceived theories to support, but simply goes to nature to find out and interpret

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what she has to say according to her exact meaning. What he may desire to be true has no bearing at all on the question what really is true. Here arises the inability of men of science to view theological questions in a light which shall be satisfactory to the theologians, and the corresponding inability of the latter to appreciate the spirit in which men of science discuss the problems of life and being. We hear much at the present time of a supposed conflict between science and religion, but it is rather a conflict between two sets of men who view nature from opposite and irreconcilable standpoints. It is essential, to the understanding of our theme, that we should see in what this difference of view consists; I shall, therefore, endeavor briefly to explain it.

The theologian looks upon the doctrines he has been taught, as something the truth of which is essential to the welfare of humanity, and, we might almost say, to the supremacy of the Creator. He thus invests them with an attribute of moral excellence, implied rather than expressed in the term orthodoxy, and looks upon those who attack them, not simply as men who are mistaken, but as men who are seeking to do a great injury to the human race. Hence, the idea of weighing the arguments on both sides with entire indifference to the result, is one which he cannot be expected to receive with favor, or even to readily comprehend as received by others. His idea of truth is symbolized in the pure marble statue which must be protected from contact with profane hands, and whose value arises from its beauty of form and the excellence of the ideas which it embodies. He therefore looks upon those who attack it, with feelings not unlike those of the keeper of the statue upon a chemist, who refuses to see anything in the statue except a lump of carbonate of calcium of peculiar form, and who wants to handle it, weigh it, determine its specific gravity and its cohesive power, and test its substance with acids. The corresponding idea of the scientific investigator is symbolized by the iron-clad turret, which cannot be accepted until it has proved its invulnerability. Instead, therefore, of being protected from violence as if it were a product of the fine arts, violence is invited. Its weak points are sought out by eyes intent on discovering them, and are exposed to the fire of every logical weapon which can be brought to bear upon them. A scientific theory may thus be completely demolished; it may prove so far from perfect that its author is glad to withdraw it for repairs or reconstruction; or it

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may be hammered into an entirely new shape. But however completely it may stand the fire, it maintains its position as a scientific theory only by being always in the field ready to challenge every new comer, and to meet the fire of every fact which seems to militate against it. A countless host of theories have thus been demolished and forgotten with the advance of knowledge, but those which remain, having stood the fire of generations, can show us a guarantee of their truthfulness which would not be possible under any other plan of dealing with them.

As a consequence of this way of viewing theories, the scientific man recognizes no such attribute as orthodoxy in his doctrines. There is nothing at all which he says you must believe to be true as a condition of scientific recognition. There may, indeed, be many propositions to doubt which would indicate extraordinary incredulity, or down-right folly, or even insanity, and he might, therefore, regard a skeptic as possessing a pitiful feeblenesss of intellect, and, in consequence, refuse to listen to him; but he would refuse, not because the man disbelieved something which was undoubtedly true, but because he was not worth listening to. Perhaps the point which I am striving to make clear may be most readily grasped by the reflection that science offers its highest rewards to him who will overthrow and supplant its best established and most widely received theories. Thus, the names of the men who disproved the theory of epicycles in astronomy, and the doctrine of phlogiston in chemistry, occupy the most honorable positions in the history of science. Of course, no such thing as authority in science has anything more than a provisional recognition. If a man of good repute says that he has investigated a certain subject and reached a certain result, the latter may be accepted on his authority, in the absence of other evidence. But this gives no reason at all why anyone else should not reach a different result, and it would be no argument at all to cite the mere authority of the first against the second. In case of a discrepancy of this kind, the whole question would have to be reinvestigated. The dictum, "It is written," has no terror whatever for the investigator of nature; he can recognize no authority for any feature in the course of nature, except nature herself as he sees her.

These principles are of so much importance in the philosophy of science, that I may be pardoned for viewing them in yet another

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light. In reading those discussions with scientific men on certain theories recently advanced by the more advanced students of philosophic biology into which the representatives of theology sometimes enter, I have often noticed, that if the representative of science propounds, discovers, or brings forward any fact or principle which seems to tell against his side of the question, the other calls it an "admission," or "concession," just as if his opponent had first selected his side for the love of it, and was then unwilling to concede or admit anything which might militate against it. Now, to go into the philosophy of the subject a little deeper than heretofore, allow me to say that the man of science professes no ability to recognize truth on sight, as he would recognize a house or an animal. The question whether any given proposition is or is not true, is necessarily to be decided by the human judgment, coordinating all the facts which bear upon it. There is no such thing as a revelation of scientific truths, and even if one should claim that there was, the admission or rejection of the claim would be an act of the judgment, which thus becomes the ultimate arbiter in any case. Hence a proposition is to be proved probable or true, not by anything in itself, but by a more or less long and painful examination of the evidence for and against it. Everything that can be found to militate in favor of it is put into one scale, and everything that can be found to militate against it is put into the other. If the investigator is imbued with the true spirit of science, his search is equally vigorous for arguments to go into the two scales. When he says that the proposition is worthy of being received as true, he means, not that it bears any recognized seal of truth, but that the evidence in favor of it entirely preponderates over all that can be brought to bear against it.

You will not understand me as maintaining that every individual man of science constantly maintains this spirit of impartiality any more than that every Christian constantly lives up to the highest standard of his profession. Hot conflicts have sometimes raged, and there is no reason to suppose that they have entirely ceased, even now, in which each combatant could only see one scale. But the spirit I have described is that in which science exhorts her votaries to approach every question, and in which they will constantly endeavor to approach it if they are worthy of their profession.

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Let us now approach our main theme, the course of visible nature. Let me again remind you that of the two universes, the seen and the unseen, I am only going to speak of the former. We find ourselves placed in this world in the midst of a vast theatre of activity. We see an atmosphere agitated by storms; great masses of water rising in the air to form clouds, and, after falling to the earth, flowing as mighty rivers to the ocean; countless forms of vegetation rising from the earth, and then returning to it; a sun supporting all life on our planet with its heat; an infinitude of chemical changes going on around us; countless stars moving through space with velocities which transcend all our conceptions. To all appearance these operations have been going on for millions of ages past, and may continue for millions of ages to come. As the thinking man contemplates them, he is led irresistably to the conclusion that they do not go on at random, but that they are joined by connecting links, or are in some way the product of knowable causes. From his earliest infancy he sees connections between them which enable him to foresee results. He finds that fire burns, that the sun warms, that food satisfies his hunger, and that heavy bodies fall with a certainty which shows the forces at play to be invariable in their action. To penetrate the mystery in which these forces are enshrouded, he has exerted the efforts of his intellect from its first dawn until the present time. What general conclusions has he reached?

From the earliest times at which man began to think, two modes of explaining the operations of nature have presented themselves to his attention. These modes are sometimes designated as the teleological, and the mechanical.

The teleological explanation of nature, presupposes that her operations are akin to human actions insomuch as they are under the control of, and directed by one or more intelligent beings having certain ends in view; that the events are so directed as to compass these ends; and, finally, that the relation of the events to the ends, admits of being discovered by observation and study. This last condition is a very important one, because, without it, the teleological explanation of the cause of nature would not be a scientific one. The doctrine that the Author of Nature has certain ends in view, and directs the whole course of events so as to bring them about, will not enable us to explain and predict the events unless we know what those ends are. But, as I have

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already said, the test of scientific advance is the power of foresight — of foreseeing what result any combination of circumstances will lead to. If we always had to wait for the result, and could then only say, I know this is the result which was intended, because it has happened, no actual foresight would be possible; and however excellent the doctrine might be as a theological one, it would not admit of being tested by observation and experiment, and the question of its truth would, therefore, not admit of being settled by scientific investigation.

You may recall the remark of a satirical philosopher, when he saw the gifts which those who escaped the dangers of a certain treacherous and stormy sea offered up to the goddess who had this sea at her command: "I see no offerings from those who were lost," said he. It was not till the voyager had got safely to shore that he found himself under the protection of the goddess.

It must be well understood that the teleological theory of nature, or, as it is now familiarly called, the explanation of natural phenomena by design, has two distinct forms, the scientific, and the theological. These forms are not antagonistic ones; the one held by scientific men, and the other by theologians; for, as you may well know, the scientific form is the one in which scientific men almost universally reject the teleological theory, while they have nothing to say against the other forms. The forms refer only to the fields to which the theory may belong, the scientific and the theological. The distinction turns, on whether we suppose the ends which the Creator has in view to be discoverable by scientific investigation, or to be inscrutable. Only in the former case have we, as scientific investigators, anything to do with the question. The theory, as we have to consider it, is in brief this:— that the course of events in inanimate nature is from time to time modified by invisible intelligences just as it is modified by man when he changes the course of a river or plants a forest.

The other explanation of nature is the mechanical one. It assumes that her processes go on in accordance with certain laws which admit of being fully comprehended by the human mind so far as their effects are concerned. Each state of things is the effect of the state which immediately precedes it, and the cause of that which immediately follows it. The course of nature is thus considered as an endless chain, of which the work of science consists in making out the forms of the links, and the modes in which

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they are connected. In this work we have to be concerned with two things; the general laws of nature, as they are familiarly called, and the facts or circumstances which determine the operation of these laws. This distinction is most clearly seen in human laws. Thou shalt not steal, is a law; that John has stolen, is a fact. The combined result of the law and the fact is that John is locked up in jail. So, that all bodies near the earth gravitate toward it with a force directly as their mass, and inversely as the square of their existence from its centre, is a universal law of nature. The Niagara river and the precipice are facts; and the cataract is the result.

But the general explanation of the course of nature, on the mechanical theory, is not of this simple kind, because the laws of nature do not act singly, but in combination; so that the result of each is modified by the action of all the others which come into play. The law of gravitation is not that all bodies must fall, but only that they tend to fall, and, therefore, will fall unless held up by some sufficient opposing force. So long as I support this weight in my hand it does not fall, because the force of gravitation and the resistance of my hand neutralize each other. But the instant I let go, the weight drops, according to a certain law known as that of uniformly accelerated velocity.

The doctrine I am endeavoring to elucidate is this: knowing a few simple laws of nature, of which gravitation is one; knowing also the arrangement of material things within the field of investigation; that is, knowing the facts, we can predict with unerring certainty what the result will be: or if we cannot predict it, it is not because of any quality of the thing itself, but only because of the insufficiency of our powers. Moreover, these results will be, as it were, another layer of facts, from which it is possible to predict new results to follow them, and so on without limit, unless some force from without intervene to change the course. If we include the whole of nature in our field, no outside facts can come in, and her course, therefore, admits of being predicted with entire certainty from beginning to end.

Now the point which I wish to bring to your attention is, the revolution which modern science has brought to pass, in the opinions of mankind, respecting the relations of the two classes of causes, or supposed causes, which I have described. That all events could be explained on teleological principles, it is not

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likely that anyone ever supposed. That the falling of heavy bodies, the running of rivers, the changes of seasons and the revolutions of the heavens, were all in accordance with mechanical laws, at least so far as the phenomena are concerned, no one ever knowingly denied. But it was thought that the action of these causes was from time to time modified by the introduction of causes of the teleological class, just as a rock might be kept from falling by the force of cohesion. The general rule has been that the more ignorant the age, the more minute and immediate was supposed to be the action of those beings who were modifying the course of nature in order to compass their ends.

As illustrating this I might commence with the age of image worship, when the fate of the individual is supposed to be at the mercy of certain spiritual entities, symbolized by forms of wood, stone, or wax. But, leaving out of consideration ideas so different from those which prevail among us, let us come nearer home. It is not many generations since men, who knew that the regular course of nature went on in accordance with mechanical laws, believed, nevertheless, that occurrences of a terrific or extraordinary character were specially brought about to compass some end of Providence. Not only so, but, what is most essential to our theme, this end was supposed to be a scrutable one. The motions of stars and planets had gone on from age to age, until no new aspect of them inspired alarm. But a comet was looked upon as a messenger specially sent to give warning of a coming calamity. The scrutable end was, in this case, the warning of mankind. Ordinary cases of sickness and accident, whatever their result, always have been looked upon as a part of the regular course of events. But it is not many centuries since the pestilence was believed to be specially sent by Heaven to punish mankind for their wickedness. Punishment and terror were here the ends which Providence was supposed to have in view. The regular daily breezes and showers were supposed to be the result of natural laws. But these laws were not supposed to be entirely adequate to the production of the tornado, which was again a special messenger, and they were suspended, or their action was modified in times of extreme drought, threatening mankind with famine.

These special messengers of Heaven have, one by one, yoked themselves to the car of natural law, so that I think I can hardly be wrong in saying that the supremacy of mechanical law, and its

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adequacy to account for the whole course of nature, as we see it going on before us, is now the almost universal opinion of educated men. This revolution in human thought is, perhaps, clearly brought out in the different view we now take of certain religious observances introduced by our ancestors, whose ideas would now be considered as approaching the irreverent. Take, for example, the prayers for the right kind of weather, which we find in our prayer-books. When they were first composed and inserted, their object was a purely practical one. As the farmers now sometimes fire off cannon to make the black cloud break and discharge its contents upon the parched field, so the prayers were to be offered up in order that the aqueous vapor in the air might be made to condense and fall. That a much more exalted view of prayer than this is now taken by the more enlightened portion of the religious world, I think we have every reason to believe.

Although we can hardly entertain a serious doubt that the mechanical theory of natural operations, or, as it is sometimes called, the doctrine of the uniformity of nature, is generally acquiesced in by the mature thought of intelligent Christendom, yet objections are frequently made to it because it seems to run counter to some of our most cherished ideas. If it were not paradoxical to make the assertion, it might be said that we hold, or at least express entirely inconsistent views on the subject. The fact is that we are pupils of two opposing schools, which are, in a certain degree, antagonistic, one of which we cannot, and the other of which we will not, give up. In one of these schools the chief teachers are observation and experience. All sentiment and emotion are banished from its curriculum, which admits only the hard realities of the outer world. The older we grow the more we see and hear of this school, and the more unreservedly we accept its teachings. It tells us that the whole course of nature takes place in accordance with certain laws capable of expression in mathematical language; that these laws act with more than an iron rigor, and without any regard to consequences; that they are deaf to prayer or entreaty, and know no such thing as sympathy or remorse; that if we would succeed we must study them, and so govern ourselves that their action shall enure to our benefit.

The other school is that of sympathy, emotion, and religious faith. In it, as children, we receive our first teachings. It shows us ourselves placed, as it were, in a forest of mystery, surrounded

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by forms over which we have no control, and able to penetrate so little into the surrounding darkness, that we cannot tell what shall happen to us on the morrow. It has, in all ages, peopled the thickets with invisible beings having an interest in our welfare or our injury, or with providential interferences designed to compass ends of which we, in advance, have no conception. Its teachings are nearest and most welcome in times of affliction and fear. Its objections to the teachings of the other school, are heard far and wide through the land. Notwithstanding the number of forms which these objections take, their essence may be condensed into a very few sentences. The following will probably be accepted as a fair rendering of their substance.

You take a contracted and unphilosophical view of nature when you say that the world is governed by inexorable laws. These laws are not governors, but only the instruments of government by which the real governor executes his purposes. With them, but without subverting or violating them, he can reward or punish, bring on prosperity or call down disaster, according to the dictates of his sovereign will. The child and the peasant call the thunder the voice of God. The modern philosopher attempts to correct them by showing that it is the product of evaporation and of atmospheric electricity. But the view of the child is really the more correct of the two, because he ascends at once to the first cause, and thus sees further than the philosopher who corrects him because the latter stops short at the immediate or secondary cause without even trying to raise his eyes to the higher source of power. I think I am not far wrong in giving this as the substance of the most cogent objections which may be anticipated in any quarter against the mechanical theory of the course of nature.

Now, if these views referred only to inscrutable first causes of things, or to the intelligent but invisible substratum which underlies the whole cause of nature, we should have no occasion to discuss them, because they would lie outside the field I have assigned as that of our contemplation at the present time, and which I have sought to describe as the field of phenomena. The doctrines that all things go on in exact accordance with the will of the Creator; that he has certain ends which the laws of nature are designed to bring about; and that an intelligent cause lies behind the whole universe of phenomena, are of a class which science has no occasion whatever to dispute. If it were made clearly to appear

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that the field of the teachings in question was thus limited, and was entirely distinct from that of phenomena, with which alone science is occupied, there would be no occasion for dispute between the two schools. I have no disposition to throw a single stone across what I consider the sacred boundary line, nor to enter a field which I am by natural and acquired habits of thought, unfitted to cultivate. As men of science let us by no means attempt to penetrate a region in which the eye of science can see nothing but darkness. If we thus subject ourselves to the imputation of being "of the earth, earthy," we may console ourselves that our edifice is firm and durable because it does not seek to rise into regions of serener air, nor to rear its dome above the clouds.

I can hardly be mistaken in saying that the objections to the mechanical theory of nature, which I have just tried to formulate, are not always confined to the field of inscrutable first causes. There is a part of the boundary line over which the stones are flying very thickly. While some of the combatants may profess to make no attack on the doctrine of the uniformity of natural law, I cannot but think that these professions often arise from a misapprehension of the scientific side of the question. Indeed, I must confess that I have met with a difficulty from my inability to form a clear idea of the views really entertained by the school now under consideration. I have made a somewhat careful study of some of the most elaborate works of the writers of the theological school, devoted to this very topic, and I have left them without being able to decide in my own mind, whether the writers do or do not hold unreservedly to the mechanical theory of the course of nature. That nearly all intelligent men really believe in this theory, at least so far as the present time and dispensation are concerned, we have abundant reason for believing. Nor is there even among advanced theologians any lack of profession of a belief in the uniformity and supremacy of the laws of nature. But, when thinkers of the other school maintain the doctrine, and trace it to its logical consequences, undisguised by sentimental language or figure of speech, they are met with criticism which I can account for only by supposing that the theologian understands by laws of nature something different from what is understood by the man of science.

Let us try to condense the questions at issue into the smallest possible space. The scientific philosopher maintains that the natu-

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ral course of events goes on in invariable accordance with certain knowable laws. He asks the theologian in the words of Pope:

> "Think'st thou like some weak Prince the eternal cause
> Prone for his favorites to reverse his laws?
> Shall burning Ætna, if a sage requires,
> Forget to thunder and recall her fires?
> On air or sea new motions be impress'd,
> O blameless Bethel to relieve thy breast?
> When the loose mountain trembles from on high
> Shall gravitation cease if you go by?
> Or some old temple, nodding to its fall,
> For Chartres' head reserve the hanging wall?"

To all these questions the other answers, no, and thus all occasion for dispute ought to end. But it does not end, by any means; for he proceeds to criticise the views of the questioner on the ground of their narrowness, and to inform him that the Creator can, (and, by implication, that he does) so arrange things that any result he may wish shall be brought about by the action of natural laws themselves. "We do not expect Ætna to recall her fires when a sage is near; or the air and ocean to acquire new motions to preserve a saint from danger." * * * "Should these individuals not be rushing recklessly against the known laws of Heaven, or should it be the will of God to preserve them, it will be found that provision has been made for their escape, and that not through the powers of nature disobeying their own laws, but through other powers in nature opportunely interposing to stop, to turn aside, or otherwise to modify their operation."

Now, always supposing that such remarks as these are intended to apply to the domain of sight, hearing and understanding, they differ fundamentally from the scientific theory in their view of what constitutes the laws of nature. The school seems to look upon causes and effects in nature as belonging to two different classes of things. They see an immense collection of causes, to each of which the appropriate effect is tied. So long as the cause is followed by its effect, the laws of nature are satisfied. So, if the Ruler wants to reward, punish, kill, or rescue, He has only to bring into operation the appropriate cause at the proper moment; the natural effect follows, and His will is executed without any violation of the laws of nature. I am not sure that this is an exact statement of the views to which I refer; but it is the best I can gather from the study of the forms in which they have found expression. Supposing this to be the view really entertained, it is

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essentially different from that held by the scientific philosophy. The course of Nature as it presents itself to the eye of science is not a collection of isolated causes, each with its effect attached to it, but it is rather to be symbolized by a chain in which each link is connected with the link which precedes it and with the one which follows it. At each moment of time, the state of the universe is the effect of the state which immediately preceded it, and the cause of the state which immediately follows. There are no such things as distinct causes and effects, but only laws of progress which connect the successive links of the seemingly endless chain.

As an illustration of this, let us take the falling of the rock. To the mere observer there is no evident reason why it should fall at one time rather than another, he may, therefore, feel that there is room for speculation as to the cause which made it fall at the exact moment it did. But science teaches that it will fall at the very moment when the cohesive extraction which binds it to the mountain behind, becomes less than the weight of the rock. We might suppose a power to so adjust the causes which effect the cohesion that the rock shall fall at some desired moment. But any such adjustment would be as complete a change of the course of nature as if the power should hold the rock up after it had begun to fall. The natural processes by which the cohesion of the rock is slowly diminished, though largely hidden from our view, are governed by laws as precise in their action as those which regulate the motion of the planets. The water which falls from the clouds slowly percolates through the ground, and enters a crack in the supporting mass. It wears it away at a rate dependent on the solubility of the material and the quantity of water which falls. A constant, but certain molecular action goes on without ceasing between each molecule of water and each molecule of rock. The strength of the latter is thus weakened according to some law admitting of precise mathematical statement. Thus, a mind possessed of sufficient mathematical ability, knowing how much water runs over the rock from time to time, and knowing also the laws of molecular action between the rock and the water, could determine long in advance the very moment at which the rock would fall.

Going back another step, we see that the quantity of water which runs over the rock depends on antecedent circumstances in

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the same way, namely, upon the quantity of the rainfall and the arrangement of the crevices in the ground. However the latter may have been produced, the cause is still another link in the endless chain which we can trace back to preceding links as far as we please. Equally is the rainfall a fixed element, determined by the course of the winds and the amount of moisture which they carry. Thus we have a network of causes, too complicated for the human mind to trace in detail, but which the philosophy of science teaches us act with mathematical certainty. No tempering, modifying, or adjusting action comes in at any stage of the process, so far as we can see; if we admit such action, we have to keep placing it farther back as our knowledge increases.

Now there is one feature of these causes, the admission or rejection of which constitutes the main point of difference between the two schools of thought which I have before indicated. All are agreed that the course of nature is determined by what we may call causes or laws, but all are not agreed as to the scope of action of these laws. The great and distinguishing feature which the school of sciences recognizes, and which the other school does not recognize, is that all the laws of nature act without any scrutable regard to consequences. I qualify my statement by the word scrutable, because it is entirely outside the pale of scientific research to speculate upon possible inscrutable ends in nature. This being a subject of which the man of science, speaking as such, can affirm nothing, so he can deny nothing. Having found that no trace of regard for consequences can be seen in the mode of action of the laws which he investigates, but that the whole course of things, so far as his eye can penetrate, may be explained and predicted without supposing any such regard, the demands of science are satisfied, and he must there stop.

Let me illustrate this by going over the train of thought which has just occupied us in the opposite direction, starting from the rainfall, and tracing the succession of causes to the fall of the rock. The spot at which each drop of rain shall fall is determined by antecedent conditions entirely, by gravitation, and the winds. The drop neither seeks nor avoids the crevices, never asks in any way what shall be its destiny after it reaches the ground. It strikes the ground wherever gravity and the winds bring it, percolates through the soil according to the law of least resistance,



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and dissolves the rock according to the laws of chemical affinity, without any respect to the consequences, immediate or remote. At length a moment arrives at which the cohesive force of the rock becomes less than the weight which urges it downward. This moment is fixed entirely by antecedent circumstances, such as the solubility of the rock and the amount of water which percolates over it. At this very moment the rock begins to fall. It falls sixteen feet the first second, three times that distance the next, and so on, according to the mathematical law of falling bodies, without any respect to the lovely character of the beings it may destroy, or the disasters with which it may crush the fondest hopes of men. The region may be the wilderness; the passer-by may be a babe in its nurse's arms, an angel of charity, fulfilling her mission of good will, or a murderer aiming the deadly blow at his victim; but under no circumstances can we see that these conditions in any way affect the chain of causes which lead to the falling of the rock, or cause it to wait a moment or swerve a hair's breadth from its inevitable course.

According to the theory of the course of nature, which I am trying to elucidate, the chain of causes which we have described, each cause acting according to antecedent conditions, but without any regard to consequences, is the type of the whole course of inanimate nature as far in space as the telescope can penetrate, and as far back in time as the geological record can be deciphered. An essential feature of the theory is, that the laws which connect the several links of the chain, and thus determine the progress of events, do not possess that character of inscrutability which belongs to the decrees of Providence, but are capable, so far as their sensible manifestations are concerned, of being completely grasped by the human intellect, and expressed in scientific language. Without this, the theory would have no practical bearing whatever, because to say that the course of events is fixed, but by laws which we can never grasp, would give us no clew at all to learning what that course shall be, and would be equivalent to telling us that it is enshrouded in the same impenetrable mystery with first causes. A very important feature of the progress of science is found in the constant resolution of the laws of nature into more simple and elementary ones, until we reach principles so simple that it is impossible to analyze them farther. Let us take

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as an instance of this the laws of the celestial motions. When Kepler discovered that the planets moved round the sun in elipses, having the sun in one focus, he found what were, for his time, simple and elementary laws. They were entirely comprehensible, admitting of being expressed in mathematical language. They enabled him to predict the motions of the planets, and, so far as the intellect of the time could penetrate, they could not be resolved into more simple expressions.

But when Newton appeared on the scene, he showed that these and other laws could be expressed in the simple and comprehensive form of gravitation of every particle of matter toward every other particle with a force inversely as the square of the distance which separates them. All the laws of planetary motion which had before been discovered were shown to be reducible to this one simple law, combined with certain facts respecting the directions and velocities of the planetary motions. The most essential of these facts is, that the velocities of the planets in their orbits are such that these orbits, under the influence of the sun's gravitation, are nearly circular.

By this grand generalization Newton reduced the laws of the celestial motions to a form so elementary, simple, and comprehensive, that no further reduction seems possible in our present state of knowledge. Attempts have been made to show that gravitation is itself the result of discoverable causes, but they appear to me entirely unphilosophical, since the causes into which they would resolve gravitation are more complex than gravitation itself. But for our present purpose it is not necessary to concern ourselves whether gravitation may arise from some more subtile principle, as yet undiscovered. The point which I wish you to grasp is the entire comprehensibility of the law, as it now stands. There is no mystery surrounding it. When I say that any body left unsupported will fall toward the centre of the earth until it meets with the earth itself, or some other obstacle to its farther fall, you know exactly what I mean, and what are the results of the law which I enunciate. In a certain sense we might say that the laws of nature are simply *general* facts, distinguished from special facts by their dependence upon certain antecedent conditions. Considered as such, there can never be any doubt as to their meaning or results. There is no profound philosophy involved in their action

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or expression any more than there is in such statements as that all unsupported bodies fall toward the centre of the earth; that gunpowder, when touched by fire, suddenly changes to an incandescent gas; that water, at ordinary pressure, changes to steam at a temperature of 212°.

Now, scientific investigators are earnestly endeavoring, each in his own sphere, to do for the whole of nature what Newton did for the laws of planetary motion, to find and announce the elementary principles which connect all the links of the endless chain which symbolizes her course. The student of chemistry cannot doubt that the innumerable properties of the various compounds which he studies arise from the play of certain attractive and repulsive forces among the elementary molecules of the matter of which these compounds are formed. Could he only learn the law according to which these forces act, chemistry might become very largely a deductive science, and the properties of compounds might be predicted in advance, as the astronomer predicts the conjunctions of the planets. The idea now entertained by those who see farthest in this direction is that all the physical properties of matter depend upon and may be reduced to certain attractive and repulsive forces acting among the ultimate atoms of which matter is composed.

It may also be supposed that all the operations of the vital organism, both in men and animals, depend, in the same way, upon molecular forces among the atoms which make up the organism. The operation of forces unknown to chemistry must, indeed, be presupposed, but there is no reason to suppose that these forces are less simple than chemical ones. Some would even go so far as to explain the facts of consciousness in this way. The philosophy of this explanation belongs, however, to another department of thought — that of scientific materialism — into which we cannot at present enter.

The most startling attempts, in the direction I have indicated, are those which are designed to show that those wonderful adaptations which we see in the structure of living animals, and which in former times were attributed to design, are really the result of natural laws, acting with the same disregard to consequences which we see in the falling rock. The philosophy of Darwinism, and the theory of evolution, will be at once brought to your mind as form-

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ing the modern system of explanation tending to this result. On these theories, the eye was not made in order to see, nor the ear in order to hear, nor are the numberless adaptations of animated beings to the conditions which surround them in any way the product of design. Absurd as this theory appears at the first glance, and great as is the anxiety to secure its rejection, the question of its truth is to be settled only by a careful scientific study of the facts of nature and the laws of hereditary descent. The principle which is to aid in its settlement is universally admitted in quarters where it is fully understood. We are not to call in a supernatural cause to account for a result which could have been produced by the action of the known laws of nature. The question then is whether these laws of hereditary descent and of natural selection are adequate to account for the gradual growth of such organs as the hand, the eye and the ear, and for all the adaptations which we see in nature. If they are, it would be idle to call in any other cause, except we place it behind the laws, and if we place it behind those laws we must equally place it behind all others. Of course, such a cause lies beyond the field of sight, and does not, therefore, belong to scientific observation. Granting the theory, then, so far as the eye of science can penetrate, the whole result is brought about by laws acting in seemingly blind disregard of consequences.

Let us now turn once more to the theory of scrutable design, which supposes at least the occasional action of causes which the human intellect can perceive to have been intended to produce certain effects, such as the salvation of the righteous, the punishment of the wicked, the warning of the indifferent, or the preservation of the race. Studying this theory from the purely scientific standpoint in all the varying forms in which history presents it, we see its distinguishing feature to be the idea of causes acting so as to bring about certain results.

When Pallas inspired Diomed with renewed strength, and gave superhuman accuracy to his aim, it was in order that he might be able to pierce his Trojan enemies. Ordinary investigation might fail to show that his hand trembled less than usual as he raised his javelin, but the goddess took care that the last tremulous motion of his hand, as the weapon left it, should be in the direction to send it into the breast of the foe. The utterances of the oracles

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were determined, not by the past or the present, but by events still in the future. The blazing comet appeared, not in obedience to a chain of causes commencing with the creation, but in order that man might be warned of the coming calamity. When the prayers of the righteous averted the coming storm, the cloud moved aside in order that their fields and houses might be saved, and when they brought down the gentle rain upon the parched fields, the rain fell in order that famine might be averted.

These supposed causes differed from what enlightened minds now understand by the term Providence, in being amenable to scientific investigation, and in not being included in the regular chain of natural phenomena. The designs of Providence are inscrutable, but those of Pallas and Juno were not. Careful experimental investigation, such as might have been undertaken by a Helmholtz of that time, would have sufficed to show just how Pallas wanted the spear thrown, if the view of the Homeric age was the correct one. When the King died, or the enemy was victorious, men thought they knew exactly why the comet appeared when it did.

These views having so far vanished into thin air, I do not see how we can avoid recognizing the reality of the revolution which modern science claims to have made in the views of men respecting the course of nature. And yet, as I have already shown, there are many tendencies in our being which make us unwilling to admit the revolution, and lead many to look upon the old theory as correct, provided it were only considered as tracing causes to the will of the Creator. On what is this view founded at the present time? Entirely, it seems to me, in ignoring the distinction between the scrutable and the inscrutable, between the seen and the unseen, worlds. Science has, to a greater or less degree, banished final causes from the visible universe; but they act with undiminished vigor in the invisible one. Such a translation may not be a great revolution in thought from a theological point of view, but it certainly is from a scientific standpoint, which considers only visible things.

I can readily imagine your asking if teleological causes can be really considered as absolutely banished from the whole domain of visible nature, if, considering how limited our knowledge, and how vast that part even of the visible universe which we do not

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know, it is not rash to assert that we know the true theory of nature, even in the field of phenomena. This question may lead us to look, a little more carefully than we have hitherto done upon the exact standing of the doctrine of the uniform course of nature according to antecedent causes, and the relation of this doctrine to modern scientific investigation. And this leads me to say that it would be entirely unphilosophical to regard the revolution I have described as a scientific discovery or induction. It may be doubted whether the scientific mind is really any less disposed to believe in final causes than the ordinary mind. Nor can the theory that the course of nature is symbolized by the chain of cause and effect, as I have described it, be considered as a product of modern investigation simply, or as belonging especially to the present age. It is a theory which has been, in a limited sphere, recognized by all men at all times. The reason why modern science has so greatly extended its scope is, that modern science has acquired a vastly more extended view of nature than has before been obtained. One of the most curious and suggestive features of the teleological theory has been that the action of teleological causes has always been ascribed to operations into which human investigation could not penetrate, although their ultimate effects might be plainly seen. Whenever the subject becomes so well understood that the chain of natural causes can be clearly followed, miracles and final causes cease, so far as the scientific explanation of things is concerned. That a ball or spear thrown in one direction would bend its course into an entirely different direction no one ever supposed. Homer never imagined Pallas as changing the course of the javelin after it had left the hand of Diomed. But those states of the nervous system which result in a certain and accurate aim, or in a tremulous or uncontrolled arm, lay beyond the pale of physiological knowledge in the time of Homer, so here it was that the goddess intervened. When nervous action became fully understood, the final cause receded and took refuge in some deeper arcanum of our ignorance. Jove was never expected to make thunder and rain without clouds, nor was the falling of the rain ever ascribed to his interference, because every one believed that if the drops were once formed they would fall at once to the ground without any action on his part. But the mixing currents of moist and cool air, and the processes of condensation which lead to the

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formation of rain and electricity, were not understood, so here Jupiter had a chance to work unseen by man. When the mode in which clouds were formed was once understood, the god of thunder left his seat upon Mount Olympus for a more distant abode. From the earliest historic times the man who took a large dose of poison has died, as a matter of course; neither good nor evil spirit had anything to do with it. But if brain disease bereft him of reason, the malevolence of an evil spirit was called in to account for the result.

Now, I beg you to notice that in all these cases, the only distinction we can make between those effects which were supposed to be produced by natural causes, and those which were produced by the will of some higher power acting with a scrutable end in view, is this: in the first class of cases we can clearly see the effect to have been produced by the action of natural causes, and in the second we cannot. This distinction, depending as it does upon the extent of our knowledge, cannot be regarded as a logical one. Yet, in so far as a belief in that class of final causes which we have been considering exists at the present day, I see no other definition of the limits within which these causes are supposed to act. Let us take an illustration from the plague now desolating our southern cities. No one would believe that under any circumstances any superior power would build a yellow fever hospital, and supply it with the best medicines. If we should say that the prayers of the whole nation for the immediate erection of such buildings would have no effect whatever, we should not be accused of unbelief or irreverence in any quarter, for every one would fully agree with us. But there are great numbers of people who believe that, if the whole nation should pray for frost, frost might be sent in answer to the prayer when it would not have come otherwise. And to many who do not share this belief, the denial of any possibility of an influence of this kind would seem to savor much more strongly of unbelief, irreligion, or irreverence, than the denial that Providence would build a hospital without human hands.

And yet, if the scientific philosophy be correct, the providential production of frost would be as miraculous and as incredible as the providential erection of a hospital in a single night without human hands. The temperature of the air and the amount of

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moisture it shall have in any given place, a day, a month or a year from the present time, is as completely fixed by the present state of things and by the laws of evaporation condensation and motion of gasses as are the position of the heavenly bodies. The first deposition of frost will be determined by forces now at play, and any deviation from the inevitable action would be a miracle of the same kind as pieces of timber hewing themselves into shape and putting themselves together untouched by man. Please notice that this similarity between the two states of things is entirely independent of any philosophical theory of natural causes. All we claim is that the laws which determine the motion of the air, the formation of clouds, the fall of rain and the deposition of frost, are, with respect to their certainty of action, of the same class with those which determine the position, the movements, and the cohesion of a stick of timber. If you claim that both classes of causes are the acts of the Creator, we have nothing to say against it. All we say is that you must interpret his acts in the same way in the two cases. You must not claim that He will produce heat or cold by a fiat of an arbitrary will, unless you also claim that He will build the hospital or leave it unbuilt, according to a similar fiat. Nor is it of any avail to say that you know it to be His will that the hospital shall remain unbuilt unless man undertakes it. We can in reply maintain that we know it to be His will that the course of nature shall go on unchanged, no matter how it may effect human interests.

It thus appears that the dividing line between mechanical and final causes, as drawn by the human mind in all ages, has not been fixed by any absolute criterion, but only near the limits of the knowledge possessed by each generation. Science has extended the line entirely beyond ordinary mental vision, not by introducing any new theory of nature, but by extending the boundaries of exact knowledge, and with them, of the field in which, by common consent, final causes do not admit of being traced. The telescope has revealed to us a universe compared with which that known to ancients is but an atom, and geology has opened up to our view a vista of ages in which the lifetime of our generation is hardly more than a moment. And thus final causes have taken their flight from a vast region in which they before lay hid in obscurity. You may now ask, have they simply taken refuge in

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the more distant but vastly wider circumference which now marks the boundaries of our knowledge, or are we to suppose them entirely banished from nature? This is entirely a question of intuition, and not at all of scientific investigation. I have described the scientific theory of nature as not admitting scrutable final causes at all, but as claiming that the law of the falling rock is symbolic of all her operations. But I think this is a view towards which philosophers have always inclined. We must always expect that men will incline to this view in proportion to their familiarity with the material side of nature. At the same time it is evident to all that there must have been a beginning of things, and that nature could not have commenced herself. We have, therefore, a wide belt left between the origin of nature and the boundaries of our knowledge in which we may suppose the inscrutable cause to have acted. Here we reach questions of philosophy which lie outside of our field, and which, therefore, we cannot now stop to consider.

The exact bearing of the subject will be better understood by condensing what has already been said so as to present the whole in a brief space.

1. When men study the operations of the world around them, they find that certain of those operations are determined by knowable antecedent conditions, and go on with that blind disregard of consequences which they call law. The criterion for distinguishing these operations is that their results admit of being foreseen. They also find certain other operations which they are unable thus to trace to the operation of law.

2. Men attribute this latter class to invisible anthropomorphic intelligences, having the power to bring about changes in nature, and having certain objects, worthy or ignoble, in view, which they thus endeavor to compass. Men also believe themselves able to discern these objects, and thus to explain the operations which bring them about. The objects are worthy or ignoble according to the character of the intelligences, which again depend upon the state of society. In ancient times they were often the gratification of the silliest pride or the lowest lusts.

3. As knowledge advances, one after another of these operations are found to be really determined by law, the only difficulty being that the law was before unknown or not comprehended, or that the

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circumstances which determined its action were too obscure or too complex to be fully comprehended.

4. Final causes having thus, one by one, disappeared from every thicket which has been fully explored, the question arises whether they now have or ever had any existence at all. On the one hand, it may be claimed that it is unphilosophical to believe in them when they have been sought in vain in every corner into which light can penetrate; on the other hand, we have the difficulty of accounting for these very laws by which we find the course of nature to be determined. Take, as a single example, the law of hereditary descent. How did such a law—or, rather, how did such a process, for it is a process—first commence? If this is not as legitimate a subject for inquiry as the question how came the hand and the eye into existence, it is only because it seems more difficult to investigate. If, as the most advanced scientific philosophy teaches, creation is itself but a growth, how did that growth originate? We here reach the limits of the scientific field, on ground where they are less well defined than in some other directions, but I shall take the liberty of concluding my remarks with a single suggestion respecting a matter which lies outside of them. When the doctrine of the universality of natural law is carried so far as to include the genesis of living beings, and the adaptations to external circumstances which we see in their structure, it is often pronounced to be atheistic. Whether this judgment is or is not correct, I cannot say, but it is very easy to propound the test question by which its correctness is to be determined. Is the general doctrine of causes acting in apparently blind obedience to invariable law in itself atheistic? If it is, then the whole progress of our knowledge of nature has been in this direction, for it has consisted in reducing the operations of nature to such blind obedience. Of course, when I say blind you understand that I mean blind so far as a scrutable regard to consequence is concerned—blind like justice, in fact. If the doctrine is not atheistic, then there is nothing atheistic in any phase of the theory of evolution, for this consists solely in accounting for certain processes by natural laws. I do not pretend to answer the question here involved, because it belongs entirely to the domain of theology. All we can ask is that each individual shall hold consistent views on the subject, and not maintain the affirmative of the

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question on one topic, and the negative on another. My object in presenting the views I have has been, not so much to propound a new theory as to promote consistency, precision, and independence of thought among those who discuss the subject.

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# REPORTS OF COMMITTEES.

## Report of the Committee Appointed to Memorialize Congress and State Legislatures regarding the Cultivation of Timber and the Preservation of Forests.

The Special Committee appointed at the twenty-second meeting of this Association, to memorialize Congress and State Legislatures regarding the cultivation of timber and the preservation of forests, would respectfully report: —

That a bill, identical in its provisions with that prepared by the Sub-committee mentioned in our report at the Hartford meeting, and which had been favorably received by the House Committee upon Public Lands, in the Forty-third Congress, was again introduced upon the motion of the Honorable Mark H. Dunnell, of Minnesota, early in the first session of the Forty-fourth Congress, and received the same reference as before.

The Chairman of your Committee now reporting was invited to present its claims before the House Committee upon the Public Lands, and the full time of one of their sessions was spent in considering its provisions. Although several of the members expressed opinions favorable to the subject, the Committee made no report, and in view of the issues then pending, on the eve of a presidential election, it soon became apparent that the occasion would not be favorable for the inauguration of new measures.

Upon this, an amendment embracing some of the features of the proposed bill was offered by Mr. Dunnell, and through his efforts was incorporated in an appropriation bill then pending, under which the Commissioner of Agriculture was directed to appoint a person to prepare a Report which was to be by him transmitted to Congress,—upon "the amount of comsumption, importation and exportation of timber and other forest products—the probable supply for future wants—the means best adapted to the preservation and renewal of forests—the influence of forests



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upon climate — and the measures that have been successfully applied in various countries for the preservation and restoration of forests."

The bill embracing this provision became a law on the 15th day of August, 1876; and a few days after a commission was granted to Dr. Franklin B. Hough, one of the Committee now reporting, for the execution of this duty.

No instructions were given, beyond those recited in the act, but in a conversation had with the Hon. Frederick Watts, then Commissioner of Agriculture, the latter expressed the wish that the researches should be made as thorough and practicable as the means afforded would allow.

Under this authority, various circulars were addressed to several classes of persons whose business or studies were thought to afford occasion for knowing facts of public interest having reference to Forestry, and the replies received in answer to these evinced a general willingness, on the part of those addressed, to aid in the collection of information of practical value in their several lines of business, or fields of observation. Among the subjects of inquiry were: — the production and use of charcoal for metallurgy, — the use of wood for ties and other purposes, by railroads, — the production and consumption of bark and other tanning materials, — the results of tree-planting in the prairie states, — experiments in the introduction of species not indigenous, and statements of the distribution, relative size, and abundance of native timber trees.

With respect to the importation and exportation of forest products, the Reports upon Commerce and Navigation, which have been published annually since the organization of the present government in 1789, afford all the statistics that we can ever expect to have; and although changes in the headings of tables (sometimes made unavoidable by changes in the revenue laws), prevent some important commodities from appearing in continuous order through the whole period, these reports nevertheless afford the data for most instructive generalizations. The opportunity was improved to its fullest extent, and tables were carefully prepared, not only showing year by year the statistics reported, but various summaries by coasts and grand divisions, and by countries and colonies, with the view of ascertaining how these exports were affected by time, the exhaustion of material, or the opening

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up of new sources of supply, or of new foreign markets, or the changing vicissitudes of trade, and the fluctuating exigencies of supply and demand.

In respect to the measures adopted for the maintenance of supplies in foreign countries by the cultivation of forests, inquiries were addressed to the Forest Administration of several European governments, and in every instance a courteous and sufficient reply was received, either in the way of written statements, or by the sending of documents affording the desired information, or by reference to official reports within reach.

A circular in the German language was addressed to each of the Schools of Forestry, and Societies of Foresters in the countries where that language is chiefly used. These embrace the greater number of these schools and societies that exist. From nearly every one of these replies were received, either as written memoranda, or printed reports, affording the facts desired.

Early in December last, a Report was presented to the Honorable William G. LeDuc, now Commissioner of Agriculture, and after due examination it was transmitted by him to the President, and laid before Congress. It was referred in each House to the Committee upon Agriculture, the manuscript going first to the House of Representatives.

The House Committee, after spending several weeks in examining the subject-matter and estimating its extent, and cost of publication, unanimously reported in favor of printing an edition of 100,000 copies. Their estimates included the statistical portion, and fixed the size of the volume at 1,100 pages, which included an allowance for numerous graphic illustrations, chiefly relating to the tabular part.

The Report then went before the House Committee upon Printing, where, from motives of economy, it was decided to omit for the present the statistical tables, and to limit the extent to 650 pages. Under this restriction, the edition was fixed at 25,000 copies, and in this form it passed the Senate Committees, and without further change was ordered for printing.

The restrictions upon the extent of the volume rendered it necessary to omit altogether the articles upon Forest Administrations and upon Forest Associations in Europe, and to condense other portions which were deemed essential to a satisfactory report. The law under which the appointment was made has not therefore

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been fully complied with, and another Report will be presented at the next session of Congress, in which other subjects specified in the statute will receive attention.

It may be proper upon this occasion to notice some points in reference to the cultivation of forests within the United States, and to compare the conditions existing here with those found in European countries where the greatest attention has been given to this subject.

The lands devoted to forest culture in Europe may be divided into four classes, according to the tenure of their title: —

1. Lands owned by the government, including in some countries separate crown-estates, appanages assigned to members of royal families, and lands set apart for certain public uses.
2. Lands belonging to communes and other local municipalities.
3. The property of churches, ecclesiastical establishments, and public institutions.
4. Private estates.

Among the first duties of an intelligent government is the protection of its own property against waste, and the maintenance of its value. If it affords supplies needed in the public service, measures should be taken, if possible, for their renewal; and in proportion as these supplies are essential to the public welfare, or difficult to procure from abroad in time of need, the task becomes one of great responsibility, and a proper subject of timely prevision and careful attention.

Hence in every country in Europe where the government owns forest-lands, systems of management, protection and renewal have been adopted to greater or less extent. These systems vary in different countries; in some being little more than prohibitions against trespass and waste, while in others they include a most careful attention to every circumstance affecting the welfare of the growing forest-systems of inspection to insure strict adherence to the plans that may take a century or more to mature, and the most thorough provision for renewal, when this growth shall have been taken off at its full period.

In respect to the second and third of the classes we have mentioned, the care bestowed by the forest administration varies according to circumstances; in some cases amounting to absolute control, and in others to a mere general supervision of the local authorities in charge, and a consent to measures that they may

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propose. The woodlands belonging to communes and local public bodies are very often burdened with rights of common usage, originating in ancient customs or special grants, and more or less embarrassing to the forester. The inhabitants of a commune may have rights of pasturage, or of feeding swine, or of gathering leaves and acorns, or of cutting wood for building purposes or fuel; and some of these wants *must* be supplied, or great hardship will be endured.

The management of these common rights renders it necessary to establish rules according to circumstances, and involves systems of regulation that have no interest beyond the jurisdiction where they apply. In many cases the government has found it necessary to terminate by purchase, or otherwise, certain common rights that interfere with the future welfare of a forest, such as those of pasturage, or the gathering of leaves, seeds, and fruits.

As a general rule, the Forest Administration has no concern with private woodlands. To this there are a few exceptions, as, for example, where a belt of forest along a frontier is deemed necessary as a means of defense, or along a river liable to inundation, where materials should be at hand for repairing embankments. More recently, the fixing of sand-dunes, the erosion of torrents, the interception of violent winds, or the malarious emanations, and other cases affecting the general interest, or the public health, have come to be regarded as proper subjects of regulation by government, although the lands may belong to private owners.

But with these exceptions the owners of woodlands generally have the right to clear or plant as they please, unless forbidden on the ground that a public injury may ensue. They may determine the species planted, fix the period of cutting to suit their own interest, and deal with their own as they may think proper, so long as it does not affect the rights of others.

It has been remarked very generally, and in all countries, that individual proprietors are jealous of interference by public authority with the rights that they regard as their own; and even where advanced ideas in respect to forest management prevail, as in Prussia, it is only within a very recent period that measures have been taken for restricting these rights, upon appeal to the public authorities, where the particular interests of others were concerned, and for uniting under one control the management of adjacent woodlands belonging to many private owners, although a



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minority in interest might dissent. In fact, the case is scarcely different with woodlands than with cultivated fields bearing annual crops, and it would doubtless prove as difficult to control the planting of private woodlands generally, as it would be to prescribe the kind of grain that should be sown in fields, or to regulate the time of harvest.

Turning from this view to the conditions that prevail with us, we find but two classes of landed property to consider, in relation to forest culture:—

1. The Public Lands owned by the general government, and in some instances by states.

2. Private Lands, including those granted to corporations, and managed by their trustees, being as fully under their control as if held by individuals.

With respect to the Public Lands, the most that can be attempted at present, is to establish regulations for the prevention of waste, and perhaps systems of leasing, or of selling timber rights, as in Canada,—the title of the land still remaining with the government, with a remote and uncertain chance of its escaping fire and waste until another crop of timber shall have grown. Measures tending to ensure reproduction can readily be applied in such cases, and the most approved methods of restoration, as practiced in Europe, may be adopted—the main question to be considered being that of the cost, and the profit of the enterprise.

It is also clearly within the power of government, in all future sales of timber lands, to annex a requirement, as a condition of the title, that a certain percentage shall forever be kept in woodland. It may also, in selling prairie land, require that a part shall be planted with timber, and be kept forever devoted to this use. These conditions would be evidently as valid as those in which mineral reservations are made and other conditions are imposed.

As to placing any new restrictions upon lands already held by private owners, this is wholly impracticable by any proceeding short of resumption of title under right of eminent domain, and payment to the owners of the full value. The re-purchase of land and its cultivation by the government as forest, would scarcely find favor with the statesmen of our day.

Since forest products are bulky, and must necessarily reach the markets by water navigation, or by roads easily guarded, their exportation beyond the limits of a given district can be readily

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controlled. Against local use and waste, it is most difficult to provide, and with a public sentiment favoring this common use, laws, however stringent, can scarcely be enforced.

As regards private lands, including lands in charge of directors or trustees, the future prospects of forest culture appear to depend upon interest alone, and these owners will manage their estates as they find it to their present advantage or future profit. It is reasonable to suppose that they will be quite as jealous of this right as they have shown themselves to be in countries where less personal freedom is enjoyed, and that the time is very remote in which any public authority may dictate when a given field shall be planted with timber trees, or when it shall be cleared. Fortunately we are wholly free from the embarrassment arising from rights of common usage that have so grievously tried the patience of European foresters, as the title of our private lands is absolutely vested in the owners.

We regard the time as near, when the question of planting for profit will very generally engage the attention of the owners of land, not only in the prairie regions, where the want of timber is among the first that is felt, but also in the older portions of the country, where the supply of woodlands was formerly in excess, and where habits of improvidence and waste must be discontinued, and methods of economy and prudent forethought must be learned.

The discussions that have been had in Agricultural and Horticultural societies, and through the public press, have done something, and should hereafter do much more, towards bringing this subject prominently into notice. In the State of Minnesota a State Forestry Association has been incorporated by law, and means have been placed at its disposal for stimulating competition by way of premiums,—not forgetting in this to excite emulation among the children of the public schools, and rivalry between counties, in the work of forest tree planting, and the formation of avenues of shade trees along the highways.

In many villages in Connecticut and other eastern states, Village Improvement Associations have been formed, in some of which the benefits sought are not limited to the material adornment of their localities, but to certain other means of intellectual culture and social refinement, which tend so much to add enjoyment to life, and to elevate and improve the condition of society generally.

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The tendency of increasing prices in any class of commodities naturally leads to inquiries into the means by which the supply may be enlarged, and cannot fail to impress the important truth that seasonable provision should be made against coming wants. When this fact comes to be generally realized, we cannot but hope that the owners of land will see it to their interest to provide against future wants by timely investment in sylviculture, sufficient at least for their own use, and that incorporated companies will find in this an inviting field for investment for market-supply.

In this work of promoting forest-culture, something may doubtless be done by State governments, but rather in the way of incidental encouragement than by direct control. They can offer premiums to be dispensed by agricultural and other societies; they can exempt planted woodlands from taxation, or from increased assessment on account of forest growth, and they can enact laws for protecting woodlands against careless fires and other avoidable injuries; they can require and regulate the planting of trees along the highways, and in places where the public interest demands; they can provide for some instruction upon the principles and methods of sylviculture, to be taught in the public schools, and can aid the higher institutions of the state in a more thorough course of instruction in this department of applied science; they can establish experimental stations, and encourage or direct inquiries tending to the discovery and dissemination of knowledge of practical value, and they can provide general laws for the formation of companies for planting and improving waste lands by forest culture.

In respect to Forestry as taught in special schools in Europe, we have to remark, that while everything relating to methods and management,—the influences that favor or retard the growth of trees, or that affect the quality of their wood,—and every means by which labor can be lightened, or profits increased, has immediate value to us. We have little to learn from their forest jurisprudence, their codes, or methods of administration, because their laws and circumstances are altogether different from ours.

At present we could give no employment to a class of men having the qualifications of those that graduate at the first-class Forest academies. We need a more general practical knowledge among the greater number, rather than the higher special training of a few.

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The experience of schools of Forestry in Europe is not without interest to us, and we should avail ourselves of the lesson which it teaches without going through the process of proving.

In most cases these institutions are under the control of government, and are intended to qualify candidates for the State Forest Service, in which, when once admitted after thorough instruction and due probation, they are sure of employment through life, with provision for support when their working days are over. In some cases, schools of lower grade are maintained by associations of land-proprietors for the fitting of young men as foresters upon private estates, and aspirants for this employment can generally gain admission to the government schools. In all of these institutions practical labor in the nursery and the forest, frequent excursions under the guidance of the professors, and occasional journeys for more varied and general observation, form a regular and necessary part of the course, and tend in eminent degree to qualify the student for the practical application of the precepts of his lectures, and the lessons of the class-room.

These schools may be divided into three classes: —

1. Separate schools of Forestry, distinct from those giving other instruction, as at Neustadt-Eberswalde and at Münden in Prussia, and at Nancy in France.

2. Schools of Agriculture and Forestry, as at Hohenheim in Wurtemberg, and at the Imperial High School of Agriculture and Forestry at Vienna.

3. Separate departments of Forestry in Universities and Polytechnic institutions, as at the Polytechnic School at Zurich, and the Forest-Institute in the University of Giessen.

The tendency in Europe is apparently towards the abandonment of the first of these in favor of the second or third. Thus recently the Forest Academy at Mariabrunn, near Vienna, has been merged in the new institution we have just named in the Capital, and that at Aschaffenburg in Bavaria is being incorporated with the University of Munich. It is claimed by the advocates of this consolidation, that several of the branches of instruction required in the course upon Forestry, such as mathematics, chemistry, natural history, climatology, drawing, surveying, etc., are amply provided for in the University; that better laboratories, museums, and libraries, can be maintained and be made more widely useful in the larger institutions, and that the student while following his

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special course, cannot fail of gaining a broader view of science by contact with what is going on around him, than he could if his horizon was bounded by the precincts of an isolated forest academy. Moreover, the students in other branches of the University would get some knowledge of Forestry from what might come under their notice in that department, and the wider acquaintance thus formed would prove advantageous to all classes. A small number of special professors would be sufficient to equip any existing University for this service, and a large saving would be realized in the general result.

No school of Forestry has yet been established in Great Britian. The government has hitherto arranged for sending to the schools in France or Germany such young men as aspire to employment in the India service, but the measure has been strongly urged of adding to one or more of the existing universities the facilities needed for securing this instruction at home. The Rev. John Croumbie Brown, of Haddington, Scotland, has been particularly active in calling public attention to this important measure. We have, in our own country, the means for carrying this idea, as it may be adapted to our wants, into happy effect. Besides our colleges and universities, there is in every state of the Union a separate institution, or a department in one previously existing, in which, through the bounty of Congress, provision has been made for instruction in Agriculture and the Mechanic Arts; and upon these should naturally devolve the task of giving such a course of practical instruction in sylviculture as may prove most useful to the country and best adapted to our national wants.

There is not a college in the country in which a few practical lectures upon the importance of tree-culture, and some of the principles that should govern in the selection of kinds and their care and cultivation could not be given with advantage before the graduating class. The young men going out from these institutions become in greater or less degree centres of influence, and if impressed with the importance of the subject, they might in many ways promote the diffusion of correct ideas among those with whom they might come to dwell.

It cannot be doubted but that the American farmer learns much from example. His ideas of domestic architecture, of improved methods of cultivation, of the value of new implements of husbandry, or in any departure from the routine of former ways, are

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largely derived from what he sees in successful application around him. A good example becomes a model for his imitation, and in this way a neatly planted avenue of trees along the highway, or a thrifty and well kept grove, may often suggest the benefits that he can also enjoy, and so the example is repeated, to the manifest profit of the owner, and to the general good.

Your committee has not hitherto attempted to memorialize the State governments upon the subject with which it is charged, deeming it as first desirable to collect information upon the existing legislation of the several states, and to gather the results of experience as affording suggestions worthy of attention. The existing laws for the encouragement of tree-culture in the different states are presented in full in the Congressional Report above alluded to (pages 200 to 213), and excepting those relating merely to the planting of trees by the roadside, or to the protection of trees planted for ornament, they may be briefly stated as follows:

In *Illinois*, the Board of Supervisors, or Court of County Commissioners in any county, may offer a bounty of ten dollars per annum for three years for every acre planted with forest trees. They are not to be set more than ten feet apart, and must be kept properly cultivated.

In *Iowa*, the sum of one hundred dollars per annum is exempted from taxation for ten years, for every acre of forest trees planted and cultivated, the trees being not over eight feet apart. The Board of Supervisors may increase the exemption to 500 dollars per acre, except for state taxes.

In *Maine*, cleared land, if planted with forest trees, is exempted from taxation for twenty years, the trees being set not less than 2,000 to the acre, and kept in thriving condition.

In *Minnesota*, a bounty of two dollars per acre is offered for ten years for planting forest trees (excepting the black locust) on prairie land, the total sum in one year not exceeding twenty thousand dollars in the state.

In *Missouri*, a like bounty is offered for fifteen years, the allowance to begin three years after planting, and the grove being kept in growing condition, certificates and proofs being renewed annually.

In *Nevada*, a state bounty of ten dollars per acre for twenty years is offered for planting done within ten years after the passage of the act (of 1877), the payments commencing two years after planting.

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In *Wisconsin*, every land owner having five acres or more of land, who shall reserve from the natural growth, or who shall plant, not over a fifth part of the area in tree-belts, is exempted from taxation on lands thus occupied, until the trees become twelve feet high, and after that he is to have a premium of two dollars per annum per acre. The position and width of the belts, kinds of timber allowed, etc., are prescribed in the act.

In *Kansas* and *Nebraska*, after an experience in the offering of bounties and exemption from taxation, the laws for this object have been repealed. We are not informed as to the reasons that led to the repeal in Kansas, but in Nebraska a revised constitution forbade all exemptions of private property from taxation, and thus rendered the previous law a nullity. It is, however, understood that opportunities for evasion and fradulent claims were too often afforded, and it was believed by some that the benefits of arboriculture were so manifest that they did not need the stimulus of a reward.

There are various questions affecting the interests of sylviculture that come within the province of state legislation, and upon which uniformity is desirable. These will be duly considered by the Committee, and when agreed upon will be submitted as their recommendation to State Legislatures.

Trusting that its proceedings thus far will meet the approval of the Association, the Committee respectfully asks for a continuance of its powers for the further prosecution of the labors with which it has been charged.

FRANKLIN B. HOUGH,

*For the Committee.*

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# SECTION A,

# MATHEMATICS, PHYSICS AND CHEMISTRY.

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# ADDRESS

BY

## PROFESSOR ROBERT H. THURSTON,

VICE PRESIDENT, SECTION A.

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LADIES AND GENTLEMEN:—

I HAVE chosen a subject for this occasion which may appear a novel one to many among my audience; but it is one which has seemed to me peculiarly suited to be the theme of the annual address of a Vice-President of the ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE. I hope that its novelty may not detract from its interest. I propose to indicate the existence of a SCIENCE OF THE ADVANCEMENT OF SCIENCE, exhibiting some of the facts upon which it is based, enunciating some of the laws and the principles of that science; and thus, as well as I may, presenting its philosophy. I propose to show, as concisely as possible, where and when this philosophy had its origin, and what has been the method of its growth. I desire to show what should to-day be done for this science, to indicate what seem to me the best methods, and what system is likely to facilitate most effectively its progress.

Science—which has been defined by Fleming as "knowledge, certain and evident in itself, or by the principles from which it is deduced or with which it is certainly connected"—consists of two parts. As Sir William Hamilton has stated it, science is a "complement of cognitions, having in point of form the character of logical perfection, and in point of matter the character of real



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truth." While it is the foundation of all art, it is itself founded upon a precise and sure knowledge of certain of the great facts of nature, and upon man's power of detecting and of revealing the great truths which have been established by the Creator of the universe. These two parts are:—

1st. Those pearls of truth which man has been able to gather, and

2nd. Those chains of laws and principles upon which those facts are strung, and by which they are firmly connected, as the pearls of a lady's necklace are bound together by the silver thread or the golden wire which gives the precious treasure its continuity.

All science is thus made up from the infinite number of facts which are comprehended in the universe of the known, and the to-be-known. Its existence is assured by the immutability of all those principles of philosophy which are woven into the connecting chain.

The man of science,—the philosopher whose task it is to create and to advance all human knowledge of the great kingdom of nature,—is, therefore, a discoverer of facts, an observer of phenomena, a student of nature's laws. He is a systematic recorder of fact and a codifier of laws. He gathers the pearls of truth and collects the silken threads that we term principles, spins them into the cords, and works them into the chains upon which he strings his pearls; and thus a science is formed.

Thus, centuries before the Christian era, the "wise men of the East" were studying the motions of the stars, and of the sun and its attendant lesser lights, and were recording observed astronomical facts which twenty centuries later have been correlated by recently determined law.

Thus, Kepler, month after month, and year after year, for the whole period of an eclipse-cycle, assuming one mathematical law after another to be applicable to the relation of periodic times of orbital revolution to the mean distances of the circling planets from their central sun, tried to fit already discovered facts to his various threads of principles; and, after long years of trial, found the right thread at last, strung his pearls upon it, and proclaimed the great law which Newton confirmed when, taking another path, correlating larger facts with broader principles, the great philosopher proved to the world the omnipresence of the law of gravitation.

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Thus, again, those great astronomers and wonderful mathematicians, Leverrier, and Adams, seizing upon this Newtonian fact—the indefinite range of the force of gravity, and of the law of gravitation,—proved the, till then, unknown fact of the existence of an enormous planet, unseen by any telescope, far beyond the supposed limits of our solar system, and even pointed to the very spot, in all the wide extent of the heavens, in which the new celestial world was to be seen.

But, although scientific work has been in progress during so many centuries, it was only in the time of Galileo, and when, two centuries ago, his contemporary,

> "Bacon, at last, a mighty man, arose,
> Whom a wise King and nature chose
> Lord Chancellor to both their laws,"

and when the experimental philosophy of the first and the modernized Aristotelian philosophy of the second united to form a perfect whole—it was only then that the existing system of natural philosophy and physical sciences had a beginning.

As the peripatetic philosopher had, two thousand years before, adopted the so-called modern system in his teachings, Aristotle may, with most propriety, be called the father of that inductive philosophy of which Bacon was the first of later disciples and expounders, and under which Galileo was the first modern worker — the first systematic experimentalist.

It was only after this system of real philosophy had become accepted by learned men that it became possible for the various branches of modern science to take shape. It was only then that the discoveries of Galvani, the inventions of Volta and of Von Guericke, and the facts recorded by Gilbert, the investigations of Oersted and of Franklin, and the researches of the multitude of their illustrious successors in the present generation could give form to the now well-developed department of electrical science. It was only after this era that the application of quantitative methods of analysis by Lavoisier, the greatest of chemists, could enable his confrères and his successors to combine the facts recited by alchemists of the earlier period, the discoveries of Priestly and of Scheele and of Cavendish and of the later chemists, with the laws enunciated by Boyle and Dalton and Avogadro, to form a system of chemical philosophy which, still avowedly incomplete and even crude, covers, nevertheless, one of the most magnificent

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fields of physical science. It was only after the inductive method had become recognized as the only effective method of acquiring an exact and connected knowledge of the phenomena and the laws of nature, that mathematical sciences—although originating as they did in an unknown antiquity, and given form by an Indian philosopher and a black slave, by the Romans, the Greeks, and the Arabians, shaped and perfected in later times by Descartes, Newton, and Leibnitz—could be made applicable to investigations by natural philosophers in every department of their magnificent work.

The richest fruits of this great system are familiar to all and have nearly all been developed within the memory of many to whom these words are addressed. The facts revealed by the researches of Rumford, Davy and Joule have been grouped and systematically united by the laws revealed by Rankine, Thompson, Clausius, and other scarcely less eminent men; and the science of thermodynamics, which has thus been created, has been applied and put to the proof by Hirn, and a hundred other distinguished engineers of our own time.

Finally, it is only now that it has become evident that this last is but another branch of the universal science of Energetics which governs all effective forces in all departments of science. The man is yet to be found, who is to combine all the facts of this latest and most comprehensive of all sciences into one consistent and symmetrical whole, and to illustrate its applications in all methods of exhibition of kinetic energy.

The grand principle which we are just beginning indistinctly to perceive and to recognize as underlying every branch of knowledge and as forming the foundation of all positive science seems, when stated, to be simply an axiom. The Scriptural declaration that the world shall endure until its Maker shall decree its destruction by Omnipotence is but a statement of a principle which is becoming more and more generally admitted as a scientific truth, viz.: that the two products of creation, matter and force, and the fruit of their union, energy, are indestructible.

The grand underlying basis of all science is found, then, in the principle that all that has been created by infinite power—matter and its attribute force, and all energy—is indestructible by finite power, and shall continue to exist so long as the hand of the Creator is withheld from its destruction. This law has been ad-

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mitted almost from the time of Lavoisier, so far as it affects matter; it has been admitted as applicable to physical energy since the doctrines of the correlation of forces and the persistence of energy became accepted by men of science, and we are gradually progressing towards the establishment of a law of persistence of all existence, whether of matter, of force and energy, or of organic vitality, and perhaps even to its extension until it includes intellectual and soul life.

The truths of science are thus coming into evident accord with those doctrines of religious belief which are common to all creeds. We are, however, as far as ever from the determination of the question whether those higher forms of force and energy have quantivalent relations and intertransformability; although a belief that mind and matter have a certain identity, and that in matter can be discerned "the promise and potency of all terrestrial life," has been avowed, explicitly or implicitly, by more than one great thinker when wandering into the realms of speculation.

Looking back to a beginning, we see, then, that in the beginning there entered upon an existence of indefinite duration a great universe of matter endowed with its characterizing attributes — the forces. These forces, acting upon a definite quantity of matter with definite intensity, give origin to a fixed amount of actual energy, and become capable of producing another fixed quantity of what is now potential energy. Energy thus brought into existence remains constant in total amount as the quantity of created matter remains constant.

The action of these forces upon this matter has given rise to every phenomenon, which has come, or which can come, within the range of scientific inquiry. But all known forces are seen at once to be capable of classification, according to their methods of affecting matter, into three great classes: —

1. Those forces with which we are able to make ourselves so readily and thoroughly familiar that we find no difficulty in assigning to each of them its proper place in the scheme of scientific systematization, and which we have found it comparatively easy to distinguish by their peculiar and readily observed effects. These include the familiar physical forces, as gravitation, electrical, chemical, and mechanical forces.

2. The vital forces — those which are preservative of all life, which produce and promote the growth of organisms having life,

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and which are less easily understood, more difficult to study, and far less subject to the modifying power of human action, than are those of the first described class.

3. The forces of the soul and of the intellect — those most wonderful and most mysterious of all known forms of force, — forces of the nature of which we know nothing, and of the effects of which, actual and possible, we have the least comprehension.

4. That master-power, Omnipotence, which is the source and the sustainer of all forces and of all existences.

By the study of the universe as it now exists, philosophers are led to perceive that its present state is such as would have resulted had the various forms of matter with which we are surrounded, and of which we ourselves are corporeally formed, and had other existences which we suppose to form a part of our universe, been, at the beginning, so distributed by the Creator and so placed in reference to the several kinds of forces that the former, acted freely upon by the latter, should, by a continuity of never-ceasing, ever-progressing change, take those infinite variations of growth and all that inconceivable variety of shapes that have been supposed to have been, by the process called "evolution," brought into the visible universe; and which still continue to illustrate the mighty and incomprehensible power of Him who is the Author and Preserver of all. It is thus apparently under the action of these forces upon this matter that the Creator is forming a universe out of chaos, that the seen and the unseen wonders of all worlds are exhibiting the marvellous intricacy of his plans, and the yet more marvellous simplicity of the means by which He accomplishes His inscrutable purposes.

Studying the accessible universe as far as we are permitted in greater detail, we find that each of the various kinds of forces set at work to modify the position and character of matter has a special part to play, a peculiar work to do; we find that the first class has a sphere of operation which is fully within the reach of our senses; that the second class of forces is also, to a certain extent, familiar to us through a knowledge of their effects; but the last two of these several classes of forces existing in nature are, as yet, quite beyond our ken. We are too utterly ignorant to do more than speculate, and our speculations, like nearly all speculations, will find no more directly profitable result than those of our Greek predecessors in that "unknowable" field.

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There is no beginning, even, in a real scientific, moral, and intellectual philosophy. We have found no quantitative measures of those forces or of their effects, and no definite principles are known which control their action. Those forces must, seemingly, be left entirely to our successors. The last named, Omnipotence, is beyond the range of imagination even.

But, studying these forms of manifestation of force which are divided between the first two classes just mentioned, we at once perceive a distinction which is as well defined as is the line separating the two classes of phenomena to which they give rise.

The PHYSICAL FORCES—and I intend here to include the mechanical and chemical, as well as the forces which are usually alone treated of in works on physics—are capable of being observed, of being distinguished by certain readily defined qualities and of being accurately measured quantitatively. The conditions which lead to their active display are capable of being exactly ascertained, and the precise results of their operations under any given set of conditions may usually be accurately predicted. These conditions are subject to certain definite modifications by the power of man, and the changes of effect which will result from such changes of condition may be predicated. The effects which nature produces in certain cases by the action of these forces may be modified by man without entirely defeating the original tendency to bring about a certain change of mode of action of existing energy. These forces, acting alone, never give rise to the more intricate forms seen in nature. Their highest product in the whole morphological range is a crystal of more or less perfect shape, but of a form which is always of some simple geometrical class. These forces do not exhibit the play of definitely directed energy tending to effect a perfectly well defined, though remote, result. Their effects are the accidental and the incidental, so far as the more wonderful and most intricate of the operations of nature are concerned.

The VITAL FORCES, on the other hand, effect operations which human power can only touch to impede or to destroy. They have for their mission the creation of strangely complicated and curiously organized structures, in which are stored certain definite amounts of energy, and which are given a power of acquiring and of applying extraneous energy, in probably also definite amount, to the accomplishment of certain tasks. Man may modify their



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operation and may produce some change in the phenomena which they are appointed to bring about; but it is only by deranging their action. He can mar their work, but cannot directly aid them. That store of vital energy which was created in the infinite past, and which is now passing through one after another of the forms of life, new and old, which are daily, hourly, momently, coming into the field of our cognizance, and as constantly disappearing from view, is continually developing organisms of every grade from the simple life-seed, if such exist — from the basic protoplasm — to the human ruler of them all, and man can only touch to distort, to stunt, or to destroy, the beautiful, the wonderful, the incomprehensible creation. As all this comes to his wondering mind, he sees that physical life is to man an existence which he can affect only for evil, and an unimaginable enigma which he can never hope to solve.

Of these two sets of forces, the one is blind and aimless, unintelligent as to the direction of its efforts, indifferent as to its results, and is governed by laws which, under all known conditions, are as simple as they are invariable. The other set appears to act at all times upon a definite, far-reaching plan, and these forces set themselves intelligently about the production of the most elegant and intricate of designs, and the elaboration of the most wonderful and mysterious of organisms. It is only in the structures which are their work that the strange, the incomprehensible phenomena of life are exhibited to the intelligence which vainly endeavors to understand them.

In studying the universe, therefore, we are naturally led to the division of all human knowledge of that portion with which we here are concerned, into two great departments, and it is this division of the "knowable" part of nature that has determined the corresponding division of the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE into two principal sections, to which are respectively assigned the two departments of physical and of biological science.

Having thus surveyed the great field in which we are all working, we are now prepared to ask: —

How are these two great departments of science to be most satisfactorily organized for future work?

How is our knowledge in both sections to be most surely and rapidly extended?

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What is the best way to set about a work, which we may be sure shall never come to an end so long as the world endures, with greatest surety that time and energy shall not be wasted in the exploration of devious paths leading to no useful end — Cretan labyrinths to the unhappy explorer or — lost in the indirect attainment of points, to which straight and easy paths may be found?

How shall time and labor, talent and energy, find their highest reward; how most greatly benefit the human race?

We see at once that the methods properly applicable are nearly the same in both departments, but yet that there is a certain difference.

The physical section is distinctively one of experiment, one which does its work by the inductive method. It is almost invariably able to proceed to the solution of the problems which are presented to it by direct experiment, and to apply crucial tests and the touchstone of experience in every part of its work.

The other section is as distinctively a department of observation, and the opportunity to solve its problems by deducing conclusions from an experimental basis of fact is far more rare in the biological than in the physical section. It is compelled to rely far more upon simple observation of such phenomena as nature may happen to present, than in the latter. Indeed, it is almost never able to adopt the most prompt and most satisfactory of methods. Yet both methods are applicable in some degree in both divisions.

But whether the work to be done falls within the province of the experimental sciences, or within that of the sciences of observation, deduction always follows observation and experiment, and the requisites for most satisfactory advancement of knowledge are, in the main, the same.

To secure most perfectly the advancement of science in all branches, and to attain our ends with a minimum of waste of time and energy, it is evident that the first necessity is a body of skilled workers in each department proportionate in numbers and in intellectual power to the extent and the difficulty of the work.

The first step is the observation of phenomena with the most perfect and critical accuracy, and the statement of facts quantitatively and with absolute precision, even though, as is often necessarily the case, they are detached and apparently without mutual relation.

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The next requisite is the systematic arrangement and registry of these phenomena of law and of fact.

The third requirement is the grouping of ascertained facts into their natural positions of scientific relation and the deduction and expression, mathematically, if possible, of the law which binds them into some continuous and defined chain or cluster.

Finally, we must correlate these detached masses, thus identified, with the great system of science, or even, if so great an achievement is possible, into the universal, pantological system which comprehends all the phenomena of nature, and which embraces the whole code of natural law—which includes the least atom of the most minute of microcosms, as well as the mightiest of macrocosms or the universe itself, seen and unseen, to its farthest bounds.

We need, therefore, the talents and skill of the finest observers, perfected by training and directed by study,—observers urged to their highest work by that enthusiasm which can only come to that man on whom has been conferred a power and a genius which make him, despite all obstacles, despite himself even, a successful specialist.

We need the services of him to whom has been given the rare and great power of insight into the mutual relations of detached phenomena discovered by observers of all countries and of all times as well as in different departments of science. It is for him to learn what is contained in that pile of invaluable but incongruous materials, and to reveal to us their relationship, bringing order out of disorder, and stringing the pearls together on philosophic threads.

We then require the aid of men of the same cast of mind, but of even greater and rarer intellect, who shall weave all our systems of science into one grand, all-comprehending philosophy. Already has good work been done in the first two directions of scientific labor, and we have seen brave and most noble attempts made to begin even the last great task, the grand and final work.

It has been by such men that science has been created, and has been given all its development. It is by quite a different class of workers that a knowledge of science and of the benefits derived from that knowledge have been given to the people.

Men of science have created science; but it is the TEACHERS OF SCIENCE who have given the fruits of all this labor and the

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product of all this talent and study to the world at large. It is the teacher of youth working in the primary or the high school, in the college, the technical school and the university, who has made our boys and our girls, our young men and our maidens, familiar with the elements of established science, and who has given us those disciples from among whom have arisen the men we most delight to honor.

It is true that our greatest men of science have usually been teachers; and it is true that our noblest teachers have often entitled themselves to enrolment among men of science. Such men have taken up two of the most honorable of the professions and have earned for themselves a double share of glory.

Strictly speaking, however, the two fields of labor are quite distinct. To unite them is to divide the energies and to distract the mind of one who is fitted to become truly great in only one of the two departments.

A complete system which shall be a real SCIENCE OF THE ADVANCEMENT OF SCIENCE thus is seen to include:—

1. Observers, discoverers, and collectors of fact,—explorers in the field of research.

2. Systematizers of law and of acquired knowledge,—creators of philosophy.

3. Teachers of science,—expounders of the law and instructors of students of natural phenomena.

Each of these classes demands, as essential to the proper performance of its work, a certain, and often a large, amount of apparatus, and there arises a necessity therefore for an ample supply of material and assistance from those who are able to do their best work in the collection and application to scientific purposes of this needed capital.

There remains, therefore, an independent class, among whom are enrolled such men as Von Rensclaer, Cornell, Thayer, Washburn, Lawrence, Sheffield, Rose, Stevens, and others, who have thus won for themselves honor, and conferred inestimable benefits on their fellow-men. There is then a class:—

4. Men to whom belongs the honor gained by the endowment of science,—philanthropists whose work comprehends the noblest of charities and is suggested by the most intelligent beneficence.

Reviewing the history of philosophy we have seen that science began her work at a time so remote that her earliest records were

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made before the historian was competent to preserve them satisfactorily. The Chaldean astrologers were such accurate observers, that, as Draper tells us in his noble work on the intellectual development of civilization, their estimate of the length of the "Saros," the cycle of eclipses—a period of more than nineteen years,—was "within nineteen and one-half minutes of the truth." Yet, until the time of the great Ptolemy that essential want, a codification of known astronomical facts and laws, remained a want unsupplied. The East Indians studied algebra, and the Greeks studied geometry, but it required a Diophantus and a Euclid to give form to those branches of mathematical science. Aristotle declared the principles upon which all real progress in science must be based; but it was only when Bacon explained those principles, and when Galileo and Newton illustrated their application, and when the art of printing had made explanation and illustration available, that the growth of natural science actually became perceivable. It was only then that experimental science found practitioners. Previously, all scientific work had been that of observation.

Some of our methods of promoting the advancement of scientific work, however, originated at a time which antedates the Christian era. We find recorded a description of such methods in early accounts of the great Alexandrian museum, when, after the death of the Greek conqueror of a world, his overgrown realm had been apportioned among his successors, the greatest statesman of them all—that Ptolemy to whom was given the wealthy province of Egypt—founded the greatest technical school and college of the sciences which has existed during the whole historic period.

It had for its declared object the collection of all learning, the promotion of the study of all literature, and of all the arts and sciences, the stimulating of all research, and the advancement of every branch of human knowledge.

To secure the object for which it was established, agents were sent abroad into the whole then known world to collect books, and scribes were employed in the museum at Alexandria copying such as could be borrowed from their owners but not purchased from their fortunate possessors, and in reproducing copies of those intended for distribution or exchange. Philosophical apparatus, of the rude but often useful forms then known, was collected and intrusted to skilful observers and experimentalists.

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A Faculty was organized,[1] which consisted of the most learned men of the time. They were assigned to sections in each of four principal departments—astronomy, literature, mathematics and medicine. Their lectures were delivered to an immense body of students, sometimes numbering several thousands, and these lectures, collected and published, constitute a splendid monument to the great men who produced them.

It was in this splendid institution, beside which our modern universities are dwarfs in magnitude, though happily not in attainments, that Aristotle first declared the true scientific method. It was here that Euclid perfected plane geometry and left it in the form in which it is still taught in our schools. It was here that Ptolemy studied astronomy and that Eratosthenes and Hipparchus taught that grandest of all sciences of observation. It was here that the first scientific engineer, Archimedes, acquired a name that is forever imperishable. It was here that Ctesilus explained to his wondering pupils the principles of the clepsydra, illustrating the laws of hydraulics by the water clock. Here it was that Hero, following his distinguished tutor, studied and taught the principles and observed the peculiar phenomena of pneumatics and of water-pressure, and illustrated the earliest known application of the now familiar power of the real "Mist God," our modern steam giant, opening temple doors and performing many curious feats by the power of steam.

The destruction of this great university, begun by the soldiers of Cæsar's armies and completed, long afterward, by the barbarous Saracen invaders of Egypt, might well signal the approach of the "Dark Ages," and the return of the world to that state of intellectual torpor from which it had been so gloriously emerging.

But learning never dies; and the revival of science, of literature, and of art in the lands conquered and ruled by these same Saracens, centuries later, and its introduction by them into Western Europe, formed the next great era in the progress of the world toward civilization. Since then, and since Bacon and Galileo, and Newton and his contemporaries, took up the mantle let fall by those earlier men of science, knowledge has never ceased to advance with continually accelerating speed. We may now feel confident that the world will never again see a succession of Dark Ages.

[1] Draper.

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Studying the history of science the student cannot fail to see that there have been from the earliest times noble examples of such men as are needed to insure progress. There have been observers of phenomena; there have been codifiers of law; there have existed grand collections of essential material intrusted to the care of able men.

But why,—setting aside the obvious political and social hindrances,—has the advance of science in the past been seriously retarded? Why is the advancement of science to-day, comparatively rapid as it undoubtedly is, still so frequently attended with difficulty? Why is progress so irregular and so toilsome? The reason is a simple one.

There has never been created and reduced to practice a systematic method of its promotion. The right men have never been selected with discretion, and thoroughly and carefully trained for specific departments of labor in those fields and to that work which most need skilled men. Men of science have not always worked in directions which most urgently call for their attention. They have sometimes even followed an unreasoning inclination diverting them into side paths, while the great highways of knowledge still remain untravelled; and they have thus wasted powers which might have given splendid returns for time and talent properly expended.

The Endowment of Research has formed no part of a complete scheme for the advancement of science, or, at least, it has never had the attention and the time given to its procurement that it should have had. The apparatus and material supplied to scientific workers, by those to whom they have been compelled to look for material assistance, has too generally been insufficient in quantity, and too incomplete in its character, to permit the most effective use of their time. For all these reasons, and more, we are not to-day prepared to do all that we should in promoting the progress of science and of the arts which so greatly depend upon it.

It is too often the case, also, that the man of science holds himself entirely aloof from the duty, to which every consideration should move him, of aiding in every way which lies in his power in the advancement of arts by the discovery and invention of methods of application of scientific knowledge and of philosophy to the industries and the affairs of common life. Were he to do this more generally and most conscientiously, he would not only confer upon mankind still greater blessings, but he would also

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secure to science that assistance from the man of the world and the man of business, upon which he is, willingly or unwillingly, very greatly dependent. There is certainly no more cheering sign than that change which has been going on in these later years: the growing inclination of these two great classes to countenance each other, and to ask and to render mutual aid.

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Now we come to the vitally important, the practical, question: What are we to do? What methods and plans are to be initiated, and how are they to be carried out by men of science and friends of science to secure its advancement, and to aid the diffusion of knowledge and its application? How can we best acquire knowledge of facts, a comprehension of laws, an understanding of their order and their relations in nature's code? and, if so much is to be permitted us, how may we hasten that great day when the sciences, now apparently isolated and unconnected, shall be seen to form parts of one magnificent system — each interlinked with others and no one isolated — a system to which all are essential, as the perfect and symmetrical and endless links of chain armor must combine to make one complete and infrangible whole?

The method to be adopted is easily stated. It is simple, comprehensive, effective. It secures the maximum efficiency of the individual and it includes the creation and the administration of organizations in which numbers assist, by combined effort, the acquirement, and the diffusion, of knowledge, and the application of the sciences to the moral, the intellectual, and the social advantage of humanity.

The Science of the Advancement of Science dictates that we shall seek:

1. To determine what are the most promising and most important directions of exploration in the great universe of the "knowable."

2. That we shall endeavor to find young men fitted by nature and led by their own inclinations, to become successful observers, discoverers and philosophers; such men are as rare as are true poets, — aid them to gain positions in which their talents may have full scope, and assist them to make known the results of their labor.

3. That we make it a part of our work to obtain for these investigators the means of research and the material aid which are

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necessitated by that rapid and ever accelerating advance of knowledge for which we are so greatly indebted to them; to secure the endowment of schools of science, to obtain a more complete organization for schools already established and liberal provision of apparatus and material for every investigator.

4. That we seek to improve our methods of instruction in science, to introduce into our educational systems a better scientific curriculum and far more extended courses, both of pure and of applied science, and to make the position of a teacher of science a far more desirable one, viewed from the world's standpoint, than it has yet become.

5. That we make the organization and the operation of our academies of science, and of our societies for the advancement of science, far more thoroughly effective.

6. We have especially to exhibit the fact that there exists between the man of science and the man of business a community of interests; the fact that he who accumulates wealth is largely indebted for his success to him who is unselfishly revealing those secrets of nature without a knowledge of which he and the world about him would be to-day in the lowest state of barbarism and the fact, on the other hand, that it is only by the possession of such means as only wealth can furnish that science can have growth,—that the astronomer can study the wonders of the heavens, that the chemist can pursue his researches among the mysteries of molecular combination, or the physicist investigate the beautiful and the most wonderful phenomena which are illustrated in the discoveries and inventions which are now so rapidly succeeding each other.

Men of science and educated men of the world must be brought to know more of each other, and must be held in bonds of closer relationship, if we are to secure the maximum of efficiency in methods of promoting science, and of securing to mankind its greatest benefits. The endowment of schools of science must precede the gathering of the fruits of research; but the endowment of schools must be preceded by the enlightenment of the possessors of wealth, and by the awakening of their interest in scientific work. The American Association for the Advancement of Science has been exceptionally fortunate in securing an appreciation among its members of this community of interests. No existing organization has done more—perhaps it would be just to

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say, none has done as much — to bring together, in harmonious working relations, these two classes: the men of science and the non-professional lovers of science.

When such a system of promoting the advancement of science, as I have made a weak effort to describe, shall have become familiar to all for whom the philosophy of nature has an interest, and when individuals and scientific organizations shall endeavor methodically to work by it, we shall, we may expect, see a new revival of learning. Let us hope that we may see this later intellectual renaissance occurring in our own time. Let us hope that a century in which that great investigator, Sir Humphrey Davy, brought out what he claimed to be his greatest discovery — Michael Faraday — shall see even nobler work done by many later Davys and Faradays; that the century that, in its early years, saw the splendid achievements of men like Young, like Rumford and Cavendish, like Laplace, Lagrange, Fourier and Carnot, of men like Gauss and Magnus, and like our own lately lost Henry—that this century may yet prove even more prolific of such noble sons.

Let us have faith that an age which still boasts of men like Thompson and Joule, Helmholtz and Kirchhoff, of men like Pierce and the long list of his successors to the presidency of this Association — all of whom have won that distinction by their works — that such an age shall see the science founded by Rankine and his colleagues correlated to the other branches of energetics, shall see the wonderful industrial revolution inaugurated by Watt and Stephenson, and Fitch and Fulton and Stevens, by Arkwright and Howe and Richard Hoe, and by Henry and Morse and Wheatstone, completely effected; and the people, relieved from the distresses unavoidably attendant upon such great social changes, happily settled down to the peaceful and contented pursuit of their legitimate aims in life — to the peaceful acquirement of the comforts and luxuries of life, of the blessings of education and culture, and to the pursuit of all the refined pleasures which are within reach of an enlightened people.

We shall, we may be sure, then see in each advancing year more of the beneficent influence of the advancement of science. Discoveries will succeed each other with ever-increasing rapidity; the now independent sciences will interweave themselves more and more thoroughly, gradually aggregating into a shapely and perfect whole; the foundation of schools of science will become yearly

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more frequent, and systems of instruction will be continually improved, until the progress of scientific knowledge, among people seeking it, will be limited only by the rate of advance of experimental science.

It is not too much to hope, even if it be too much to expect, that those among us who may be favored with life to the end of the nineteenth century may witness wonders beside which those of our telephones, and our phonographs, of our microphones and the star-detecting tasimeter, shall seem commonplace, beside which the "Hoe perfecting press," the present locomotive engine and the mightiest of modern steamships, will appear as rude as now do to us the press of Franklin, the halting colliery engine, or John Fitch's steamboats—the wonders of the beginning of this period.

In effecting all these changes, in the production of every improvement, of every advance in civilization, of everything that aids the human race physically, socially, intellectually, and even morally, the part to be taken by the man of science has an importance which cannot be exaggerated. He is to furnish the worker in every department of life, a knowledge of the facts and of the laws of nature upon which every profession and every trade is based. His is the noble mission to study the works of his Maker. To him alone is it given to read the great book of nature—the only commentary upon God's written word—and he alone can authoritatively respond to that impressive question, "What hath God wrought?" whether asked by priest or layman or the professional theologian, and whether it has reference to the world visible to the eye and to the processes in operation all about us observable by the senses, or to those opposite infinities which are only opened to us by the microscope and the telescope.

The mission of science is one the scope of which we cannot fully comprehend and appreciate; but every worker in science and every lover of nature may at least see that it is important that we should more thoughtfully study philosophic methods of scientific advancement, and seek in every possible way to aid investigators in their work, to encourage the philosopher, to promote the work of educators, and to reward him who embodies in useful inventions the principles made known through the progress of science. It is by the study of the Science of Science-Advancement that we shall most effectively prepare ourselves intelligently to proceed to do our share of this great work.

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# PAPERS READ.

## Friction and its Laws, as determined by recent experiments. By R. H. Thurston of Hoboken, New Jersey.

Introductory.—The generally accepted "law of friction" as stated in the text-books is the following:

The resistance offered by friction where two bodies, forced into contact, slide one upon the other, is directly proportional to the pressure and is independent of the area of rubbing surface and of the velocity of rubbing.

This law, although approximately correct for solids sliding one upon another, provided no abrasion takes place, is quite incorrect beyond that limit. It is also entirely incorrect as applied to lubricated surfaces; in this case the resistance follows an entirely different law.

Where two rubbing surfaces are lubricated with oil, it is obvious that, the two surfaces being separated by a fluid film, the resistance to their relative motion is not due entirely and, perhaps, may not be at all, to friction of solids, but may be produced by the resistance of the fluid to molecular disturbance, since, with good lubrication, the solids may be kept apart and out of contact with each other by an interposed film of lubricant. It is therefore evident that the resistance to motion may follow a law intermediate between that governing the friction of solids and that of fluid friction, approaching the latter more closely as the lubrication is more perfect and the separation of the solids is more complete.

Under such conditions, two limits may be noted to the law governing resistance under ordinary circumstances:

1. When the pressure is made excessive, the unguent is forced out, and, the solids coming in contact, abrasion occurs, producing a sudden and great increase of resistance.



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2. When the pressure is diminished, a limit is finally reached at which the resistance to motion is due to the viscosity of the unguent, and this is still effective even when the rubbing parts are relieved of all pressure. The law of variation of resistance is then that of fluid friction purely.

Referring to the text-books and manuals in which this subject is treated, it will be seen that these facts are not recognized and that tables of "co-efficients of friction" are given, in which figures are presented which have been obtained by experiment under conditions which differ widely from those generally observed in the operation of mechanism and particularly of heavy machinery. The question naturally presents itself, therefore: Are the values of the co-efficients of friction there given and heretofore generally accepted, correct and invariable? are they not possibly very far from correct?

The writer determined, some ten years ago, to investigate the subject, first securing the conditions usual in practical work. It was only several years later, however, that he was able to commence the investigation, some of the results of which are to be here presented.

THE APPARATUS.—The first problem which was attacked was

Fig. 1.

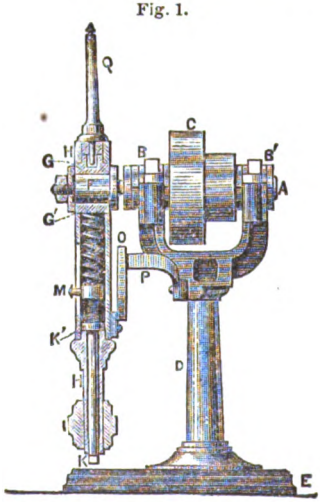
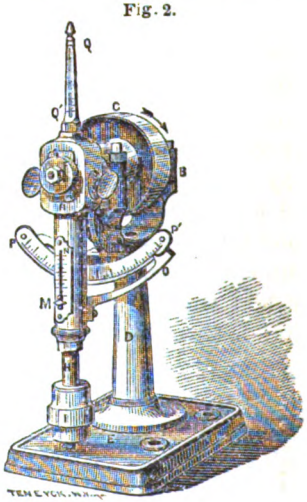



Fig. 2.




the invention of an apparatus by means of which all the conditions demanded could be secured conveniently and accurately, and the

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desired results obtained with scientific accuracy. This was done and a machine[1] was constructed as shown in figures 1 and 2.[2]

In this piece of apparatus, as devised by the writer, it was attempted to secure a means of placing the lubricant under precisely the conditions of actual service and, at the same time, to provide facilities of observing its behavior and of obtaining exact data. It was therefore made to consist of a testing-journal, *F*, which was a hollow shell carried on a spindle resting in its own bearings, *B*, *B'*, and driven at any desired speed by a cone-pulley, *C*. The hollow cylindrical test-journal, *F*, was made sometimes of iron, cast or wrought, and sometimes of steel, and was grasped by a pair of "boxes," or bearings, *G*, *G'*, of bronze or other metal which formed a part of the weighted pendulum, *H*, *H*, *I*, *I*, depending from that journal. The pressure was made adjustable at pleasure by means of a spring compressed to the required tension by a screw, *K*, and was read off on the scale, *M*, *N*, Fig. 2. This scale reads pounds per square inch of longitudinal section of the bearing on the one side, and total pressure in pounds on the other side of the pointer, *M*.

A thermometer, *Q*, inserted in the upper "brass," *G*, indicates the temperature of the bearing.

The main-spindle bearings, *B*, *B*,' are carried on a standard, *D*, and base, *E*, and the standard furnishes a support for *P*, *P*,' the graduated arc traversed by a pointer, *O*, *O*,' attached to the pendulum, *H*.

As the journal turns, the friction in the boxes, *G*, *G*,' of the pendulum, causes the latter to swing out of the vertical line until its moment of resistance is equal to the moment of friction, the pointer, *O*, traversing the arc, *P*, *P*,' and indicating the value of the frictional resistance by pointing to a figure which represents the resistance (in pounds) offered to sliding by the rubbing surfaces. This figure is the quantity, *F*, and the pressure read at the same time on the scale *M*, *N*, is the quantity, *W*, in the standard expression $f = \frac{F}{W} =$ the co-efficient of friction.

The machine can thus be made to work at any desired pressure on the test-journal, and can be driven at any speed; while, at any instant, the temperature of bearings and the friction, both the actual

[1] This machine was exhibited at the Centennial International Exhibition, 1876, by the American Society of Civil Engineers. See R. R. Gazette, 1878.

[2] From the Scientific American.

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and the co-efficient, can be read. By placing a standard quantity of each of the lubricants to be compared on the journal, and observing the periods of time, the numbers of revolutions made, or the temperature attained with each, measures are also obtained of their endurance. These machines have been employed several years in the Mechanical Laboratory of the Stevens Institution of Technology, and a large amount of work has there been done with them.

THE THEORY OF THE MACHINE is as follows:

Let $R$ = radius to centre of the weight on pendulum.
" $F$ = force due to weight of arm.
" $r$ = radius of journal.
" $l$ = length of journal.
" $w$ = weight of pendulum complete.
" $P$ = total pressure on journal.
" $p$ = pressure per sq. inch of longitudinal section.
" $T$ = tension on spring.
" $\theta$ = angle between arm and a perpendicular through axis.
" $f$ = co-efficient of friction.
" $Q$ = total friction.

When $\theta$ is equal to 90°,

$$FR = Qr \qquad (1)$$

and when any other angle

$$FR \sin\theta = Qr \qquad (2)$$

Solving equation (2) with respect to $Q$

$$Q = \frac{FR \sin\theta}{r}. \qquad (3)$$

The co-efficient of friction is

$$f = \frac{FR \sin\theta}{rP}. \qquad (4)$$

The pressure per square inch is

$$p = \frac{P}{4lr} = \frac{2T+w}{4lr}; \qquad (5)$$

from this last equation the graduations on the right hand side of the index-plate are deduced.

From the equation

$$N = 4plr \qquad (6)$$

the numbers on the left hand side are determined.

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By substituting in equation (1) the value of $Q$, in terms of the co-efficient and total pressure, it becomes

$$FR = f(4plr)r. \quad (7)$$

Solving with respect to $f$ equation (7) becomes

$$f = \frac{\frac{FR}{r}}{4plr}. \quad (8)$$

From the numerator of the second member of equation (8) the graduations on the arc are deduced.

In applying the foregoing equations to the machine seen in the engraving, the following numerical values are given to the respective symbols:—$F = 2{\cdot}5$ lbs.; $R = 10$ in.; $r = {\cdot}625$ in.; $l = 1{\cdot}5$ in.; $4lr = 3{\cdot}75$ sq. in.; $w = .6$ lbs. Also a compression of $1\frac{3}{8}$ inches of the spring corresponds to a tension of 100 pounds; hence, for each pound's tension the spring will be compressed ·01375 of an inch.

The graduations on the right hand side of the scale are obtained from equation (4),

$$p = \frac{2T + w}{4lr}. \quad (4)$$

The first graduation will naturally be that value of $p$ where $T$ is equal to 0, which value is 1·6.

On other machines in use in the Mechanical Laboratory of the Stevens Institute of Technology, other proportions were adopted. The largest machine is three times the size of that above described and is provided with a testing-journal which is a copy of the standard car-axle journal of the Pennsylvania Railroad Co.

Investigations.—A very large number of lubricants have been experimented upon in the Mechanical Laboratory, and the co-efficient of friction has been determined for a wide range of temperatures, pressures and velocities.

No special investigation of the method of variation of friction with speed has been here made until since this paper was commenced, as that problem had already been attacked by Prof. Kimball, of Worcester, in this country, and by Profs. Jenkin, Ewing and others abroad.

Friction with varying Velocity.—The last-named observers suggest that there probably exists a law of increase of friction with decrease of velocity which holds good throughout the whole



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range of velocities, down to zero of motion, where the co-efficient of friction of motion changes by a continuity of variation into that for starting from rest. The writer has, however, found the change from the one condition to the other to occur very suddenly at all ordinary speeds at the instant of coming to rest, and, as will be seen presently by reference to the table to be given, the friction *at the instant of stopping* is always much below that of starting, although greatly in excess of that for motion at all speeds in ordinary work.

Kimball gives the following[3] as illustrating the decrease of friction with increase of velocity:—

| Velocity = ft. per min., | 1 | 3 | 5 | 7 | 10 | 15 | 20 | 30 | 40 | 60 | 80 | 100 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Co-efficients of Friction, | ·150 | ·122 | ·104 | ·093 | ·079 | ·066 | ·058 | ·054 | ·053 | ·052 | ·051 | ·050 |

These results indicate that the increase of velocity of rubbing, at least with lubricated surfaces like these and under the observed conditions, causes a decrease toward a fixed minimum value, which in this case is not far from five per cent.

Since, as shown by the experiments of the writer, every change of surface, as well as of pressure, temperature, or velocity, and every change of lubricant produces a change in these values, it will require an immense expenditure of time and no inconsiderable expenditure of money to determine these effects satisfactorily. The general law above stated is, however, well established as affecting lubricated metallic surfaces.

The speed of rubbing for a railroad car-axle journal is usually not far from 250 feet per minute; that of a cotton-spindle is about 600 per minute, and the more usual velocities of rubbing in machinery are above the mean range of the above described experiments.

Experiments at the Stevens Institute of Technology.—The following are the results of a series of experiments, made at usual pressures and speeds, on the testing-machines devised by the writer, for the Mechanical Laboratory of the Stevens Institute of Technology.

Friction Co-efficients on Lubricated Metal.—Thurston.
Variation with Speed and Pressure.

*Temperature*, 110° F.

At 200 pounds per square inch.

| Velocity ft. per minute, | 30 | 150 | 250 | 500 | 700 | 1200 |
|---|---|---|---|---|---|---|
| Co-efficients, | 0·0100 | 0·0034 | 0·0035 | 0·0036 | 0·0041 | 0·0063 |

[3] American Journal of Science, March, 1878.

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| | | | | | | |
|---|---|---|---|---|---|---|
| | At 150 pounds. | | | | | |
| Velocity ft. per minute, | 30 | 150 | 250 | 500 | 700 | 1200 |
| Co-efficients, | 0·0033 | 0·0030 | 0·0031 | 0·0040 | 0·0041 | 0·0050 |
| | At 100 pounds. | | | | | |
| Velocity ft. per minute, | 30 | 150 | 250 | 500 | 700 | 1200 |
| Co-efficients, | 0·0025 | 0·0028 | 0·0031 | 0·0044 | 0·0050 | 0·0078 |
| | At 50 pounds. | | | | | |
| Velocity ft. per minute, | 30 | 150 | 250 | 500 | 700 | 1200 |
| Co-efficients, | 0·0035 | 0·0038 | 0·0045 | 0·0070 | 0·0080 | 0·0125 |
| | At 4 pounds. | | | | | |
| Velocity ft. per minute, | 30 | 150 | 250 | 500 | 700 | 1200 |
| Co-efficients, | 0·045 | 0·050 | 0·060 | 0·075 | 0·094 | 0·141 |

Tests of the journal made after this series of trials proved it to be in the same condition as at starting, showing that all the above data are probably perfectly reliable.

Studying these results and comparing them with those obtainep at a lower range of velocity and within a more restricted range of pressure by earlier investigators, it becomes apparent that, at these pressures and speeds and under usual conditions of journal surface, the friction increases slightly, but observably with increase of speed. It would thus appear that, after reaching the minimum value, as in Kimball's experiments, the co-efficient increases again, and not improbably indefinitely.

The temperature of the journal influences in a marked manner the frictional resistance, a fact which does not seem to have been previously understood, and the above series of measures were therefore all taken at the same temperature, the journal running about as warm as does an ordinary car-axle journal usually.

Friction at Varying Temperatures.—The following data were obtained by heating the bearing by its own friction to a maximum, 175° F., well within that liable to produce alteration of the oil, and noting the friction at successive temperatures while cooling. It should be remembered that no temperature readings can be taken as more than approximate.

Friction and Temperatures. Thurston.

*Steel Journals; Bearings of Bronze; Lubricant Sperm Oil.*

Velocity 30 feet per minute.

| Pressure lbs. per sq. in. | Temperature Fahr. | Co-efficients of Friction; *f*. |
|---|---|---|
| 200 | 150° | 0·0500 |
| " | 140° | 0·0250 |

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| Pressure lbs. per sq. in. | Temperature Fahr. | Co-efficients of Friction; $f$. |
|---|---|---|
| 200 | 130° | 0·0160 |
| " | 120° | 0·0110 |
| " | 110° | 0·0100 |
| " | 100° | 0·0075 |
| " | 95° | 0·0060 |
| " | 90° | 0·0056 |
| 150 | 110° | 0·0035 |
| 100 | " | 0·0025 |
| 50 | " | 0·0035 |
| 4 | " | 0·0500 |
| 200 | 90° | 0·0040 |
| 150 | " | 0·0025 |
| 100 | " | 0·0025 |
| 50 | " | 0·0035 |
| 4 | " | 0·0400 |

The figures just given would indicate that the sperm oil used in this instance and *under these conditions* works best at lowest temperatures, and that a heating journal gives rapidly increasing friction and rapidly increasing danger. At usual temperatures, 90° to 110° F., the best pressure seems to have been from 100 to 150 pounds on the square inch.

The following data were similarly obtained:—

FRICTION AND TEMPERATURE. THURSTON.

*Steel Journal; Bronze Bearing; Sperm Oil.*

Velocity 30 feet per minute.

| Pressure lbs. per inch. | Temperature Fahr. | $f$ |
|---|---|---|
| 150 | 150° | 0·0500 |
| " | 140° | 0·0333 |
| " | 130° | 0·0200 |
| " | 120° | 0·0110 |
| " | 110° | 0·0033 |
| " | 100° | 0·0028 |
| " | 95° | 0·0025 |
| " | 90° | 0·0025 |
| 100 | 150° | 0.025 |

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| Pressure lbs. per inch. | Temperature Fahr. | $f$ |
|---|---|---|
| 100 | 140° | 0·016 |
| " | 130° | 0·0049 |
| " | 120° | 0·0044 |
| " | 110° | 0·0037 |
| " | 100° | 0·0031 |
| " | 95° | 0·0031 |
| " | 90° | 0·0031 |

Here it is seen that the best running temperature, for these pressures and this slow speed, is below 100° F., and that a minimum is reached at the lower temperatures given.

The sudden and immensely great change of value observed near this limit, as at 130°–140° in the last set of figures, and at 120°–110° in those for 150 lbs. pressure, is a very striking and quite unanticipated phenomenon. It would seem that there exists what may be called a "*critical temperature*," above which it becomes very unsafe to run. This limit is not far from 100° for a pressure of 200 lbs., 110° for 150 lbs., and 130° at 100 pounds under the conditions above given.

THE INFLUENCE OF CHANGE OF PRESSURE, at constant speed, is the most marked and the most important of the variations noted by the writer. Engineers and physicists have been accustomed to apply the co-efficients determined by Gen. Morin to all cases indiscriminately, and even so eminent a writer and engineer, as well as philosopher, as Professor Rankine quotes the following without qualification[4]:—

SURFACES.

| | $\phi$ | $f$ | $1 : f$ |
|---|---|---|---|
| Smooth surfaces, occasionally greased, | 4° to 4½° | ·07 to ·08 | 14·3 to 12·5 |
| " " continually greased, | 3° | ·05 | 20 |
| " " best results, | 1¾° to 2° | ·03 to ·036 | 33·3 to 27·6 |

in which the value of $f$ for machinery is from 0·03 to 0·5; while the writer has found it to vary from a value several times greater for very low pressures, to values less than one-tenth the minimum just given for very great pressures, under conditions otherwise constant.

This is seen in the table already given to exhibit the effect of varying friction. It is still better shown that the co-efficient

[4] Steam Engine and Prime Movers; p. 15.

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varies immensely with variation of pressure in the following table,[5] which also exhibits the laws by which the co-efficient of rest and at the instant of stopping vary:—

FRICTION OF STEEL ON BRONZE, *lubricated with sperm oil.* THURSTON.

Velocity = 750 feet per minute.

| Pressure per in., lbs., | 4 | 10 | 25 | 150 | 200 | 250 |
|---|---|---|---|---|---|---|
| Value of $f$, | 0·120 | 0·080 | 0·041 | 0·009 | 0·009 | 0·009 |

*Another case.* Velocity = 250 feet per minute.

| Pressure, | 300 | 500 | 1000 |
|---|---|---|---|
| Value of $f$, | 0·005 | 0·0025 | 0·008 |

*Another case; Cast Iron and Steel lubricated with Sperm Oil.*

Velocity = 150 feet per minute.

| Pressure, | 50 | 100 | 250 | 500 | 750 | 1000 |
|---|---|---|---|---|---|---|
| Co-efficient for motion, | 0·013 | 0·008 | 0·005 | 0·004 | 0·004 | 0·009 |
| " . " rest, | 0·070 | 0·135 | 0·140 | 0·150 | 0·185 | 0·150 |
| " at stopping, | 0·030 | 0·025 | 0·040 | 0·030 | 0·030 | 0·030 |

The values at the instant of stopping are very difficult of exact determination and are not very greatly to be relied upon.

It is seen that the variation of pressure produces a very great change in frictional resistance as measured by the co-efficient. The equation expressing the law may be taken, for well smoothed journals lubricated with sperm oil, at

$$f = \frac{a}{\sqrt{P}}$$

in which $a$ has a value varying from 0·40 down to 0·05 or less with differing conditions of temperature, lubrication, smoothness of surfaces and speed. It may be taken, for all ordinary purposes, at $a = 0{\cdot}10$.

The co-efficient for rest varies in the opposite direction, increasing with increase of pressure, and

$$f' = b\sqrt[3]{P}$$

for commonly adopted conditions, in which $b = 0{\cdot}02$ pretty nearly.

The value of the co-efficient at the instant of stopping, when a sudden jump always occurs, is nearly constant. In the above detailed set of experiments, it was $f'' = 0{\cdot}03$.

Reviewing what has been stated, the following conclusions seem justifiable:—

1. That the value of the co-efficient of friction is variable greatly and often irregularly by changes in the character of the

[5] From the writer's unpublished lectures on this subject.

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rubbing surfaces, and in the method and material adopted in lubrication, and by changes in temperature, pressure or speed.

2. That the co-efficient varies usually with changes of velocity of rubbing, where smooth journals are well lubricated, nearly as the fifth root of the speed—$f \propto \sqrt[5]{v}$.

3. That its value for motion, under similar conditions of smoothness and of lubrication, with fairly approximate accuracy, varies inversely as the square root of the pressure—$f \propto \frac{1}{\sqrt{P}}$.

4. That the co-efficient at starting, *i. e.*, that for rest, varies approximately directly as the cube root of the pressure—$f' \propto \sqrt[3]{P}$.

5. That the value of the co-efficient at the instant of stopping is uncertain, but probably invariable by change of pressure—$f'' =$ constant.

6. That we may express the value of the co-efficient of friction for good journals, well lubricated with sperm oil, by

$$f = c\frac{\sqrt[5]{V}}{\sqrt{P}},$$

in which $c$ may be taken at all usual pressures and speeds for heavy machinery, in good order, as approximately 0·15, but varying as some undetermined function of temperatures as well as with pressures and speeds.

7. That each lubricant gives best results at some one temperature which is modified by change of composition and by the condition of the journal and rate of speed, as well as by variation of pressure.

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## Experimental Determination of the Velocity of Light. By Albert A. Michelson, U. S. N.

Considering the importance of this physical constant as one of the simplest and most accurate means of ascertaining the distance of the sun from the earth, it seems surprising that but three scientists have sought to obtain it experimentally.

These were Foucault, Fizeau, and more recently Cornu.

Foucault used the method known as that of "Wheatstone's re-

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volving mirror," the application of which was first suggested by Arago.

Fizeau and Cornu both used another method, known as that of the "toothed wheel."

In Foucault's experiments the distance traversed by the light was 20 meters. The result obtained was 185,200 miles per second. Cornu's stations were about 14 miles apart. The result obtained by him was 186,600 miles, which exceeds the former one by 1,400 miles.

The objection to Foucault's method is that the displacement, a quantity which enters directly in the formula, is very small, and therefore difficult to measure accurately. The objection to Fizeau's is that the time of total disappearance of the light was necessarily uncertain.

The object of the experiments which I have undertaken is to increase the displacement in the first method. This can be done in several ways:—1st, by increasing the speed of the mirror; 2nd, by increasing the distance between the two mirrors; 3rd, by increasing the radius of measurement, *i. e.*, the distance from the revolving mirror to the scale.

In Foucault's experiments the speed of the mirror was 400 turns per second; the radius of measurement was about one mètre, and the distance between the mirrors was about ten (10) mètres. The displacement obtained was about 0·8 millimètres.

In my experiments, the speed of the mirror was but 130 turns per second—but the radius of measurement was from fifteen to thirty feet—and the distance between the mirrors was about 500 feet.

The displacement obtained varied from 0·3 inch, to 0·63 inch, or about twenty times that obtained by Foucault.

With a greater distance between the mirrors, and better apparatus, I expect to obtain a displacement of two or three inches and to measure it to within one thousandth part of an inch.

The following is a description of the apparatus employed in these preliminary experiments.

Fig. 1 represents the plan. The sun's rays are reflected by a heliostat through a slit $S$, and upon a mirror $R$, which revolves about a vertical diameter. They are thence reflected to a fixed plane mirror $M$, upon the surface of which an image of the slit is formed by means of the lens $L$. The light now retraces its path,

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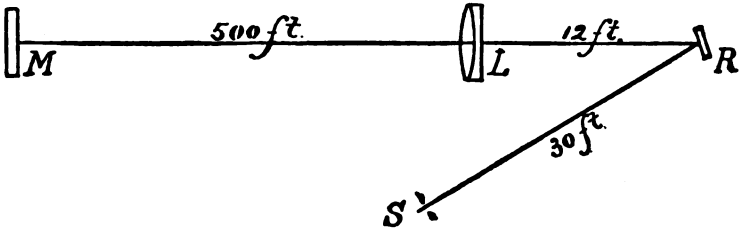
and finally forms an image of the slit, which, when the mirror, $R$, is at rest, coincides exactly with the slit itself. When the mirror revolves slowly, this coincidence is still maintained, but the image is perceived by flashes of light at each revolution till they follow each other in such rapid succession as to form a continuous impression. This image is displaced more and more as the revolution becomes more rapid, the displacement being twice as great as the displacement of the mirror, during the time required for the light to travel from $R$ to $M$ and back again.

It will be observed that the difference between this arrangement and that of Foucault, is that the concave mirror is dispensed with, its office being accomplished by the lens and plane mirror; and that this arrangement permits the use of *any* distance between the mirrors.

When the revolving mirror is in the astronomical focus of the lens, the light, reflected from it in directions continually changing

Fig. 1.




as the mirror revolves, is, after passing through the lens, always rendered parallel to the axis. Thus, all the light which comes during the part of a revolution represented by the angle subtended by the lens, is collected into a succession of pencils whose axes remain parallel to the axis of the lens; hence the distance between the mirrors may be as great as we please.

It is to be remarked, however, that as this distance is increased, the radius (or distance, $R\,S$, Fig. 1), must be decreased, if we wish to preserve the maximum of light. In practice, it was found unnecessary to place the revolving mirror at the astronomical focus of the lens, and, in fact, this distance was in some cases only one-fourth the focal length of the lens.

Other things equal, the greater the diameter and the focal length of the lens, the brighter will be the image.

In these experiments the lens and the fixed mirror were those

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used in one of the expeditions for observing the transit of Venus. The lens was five inches in diameter and thirty-nine feet focal length. The mirror was tested and found to be almost exactly plane. For these experiments it had to be silvered on the front surface.

The lens being in position, the mirror, which had a slow motion in two planes, was adjusted perpendicular to the line passing through their centres, as follows:—

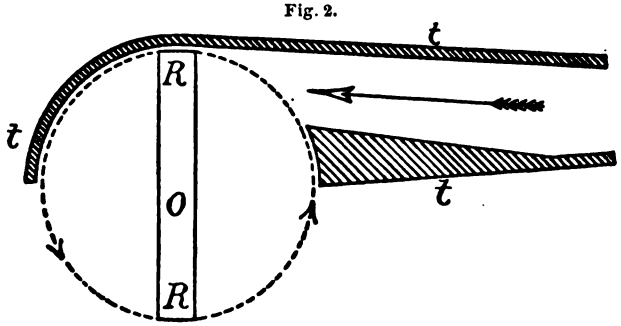
A rough adjustment was made by sighting along a square applied to the mirror, and then an observer, with a telescope placed behind the centre of the lens, saw, reflected in the distant mirror, the image of some adjacent object. By signals the mirror was moved till the observer saw the reflection of his telescope exactly in the centre of the mirror, when the adjustment was complete. This adjustment had to be repeated from day to day.

The revolving mirror was next brought into position, and then the slit placed so as to allow the beam of light passing through it to fall on the revolving mirror, and at such a distance that the image of the slit was superposed upon the slit itself.

The revolving mirror was a disc of plane glass about one inch in diameter—silvered on one side, and supported by two screws, terminating in needle points, which fitted into two small conical holes in the edge of the disc.

It was driven round by a blast of air from a bellows, which impinged upon one-half of the mirror. *t*, *t*, *t*, Fig. 2, represents a

Fig. 2.




section of the tube supplying the air. *R*, *R*, is a section of the glass disc; and *O*, is the axis about which it turns.

This crude piece of apparatus is now supplanted by a turbine wheel, which insures a steadier and more uniform motion.

For keeping the speed constant and for measuring it accurately

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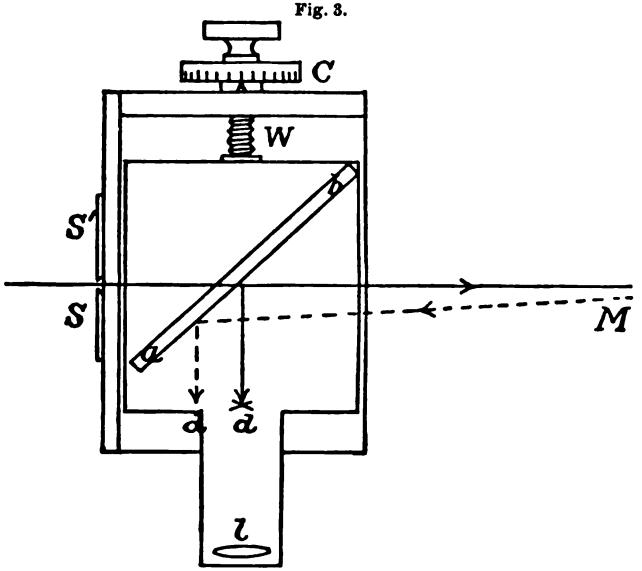
two devices were used. In the first the light reflected from the revolving mirror fell upon the toothed wheel of a chronograph, which itself was accurately timed after each experiment.

When the time between the passage of two adjacent teeth was the same as the time of one revolution of the mirror, then the wheel appeared stationary, turning slowly forwards or backwards as the mirror revolved too slowly or too rapidly.

In the latter part of the experiments, a tuning-fork, bearing a mirror on one prong, was used. This was kept in vibration by a current of electricity. The fork was placed so that an observer, about

Fig. 3.




to measure the displacement, could also see, in the mirror attached to the fork, an image of the revolving mirror. When both mirrors are at rest, this image is of course similar to the object; but when the fork is set in vibration the image is drawn out into a band.

When, however, the revolving mirror is started and attains the proper speed, the image again assumes its original shape. The fork is afterwards compared with one of Königs standards.

The slit and micrometer were connected so as to form but one piece. This piece of apparatus is represented in Fig. 3. $S$, is the slit. $ab$, a piece of plane glass partly silvered. The light proceeding in the direction $SM$, is returned in the contrary direction, and part of it is reflected from the glass $ab$, forming an image of

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the slit which is made to coincide with the cross-hairs at $d$. When the mirror revolves this image is displaced to $d'$. The slit $S$ may then be moved in the direction $SS'$ till the *displaced* image coincides with the cross-hairs, and the distance $SS'$ accurately read, by a screw bearing a divided circle. This method of observing does away with any error which might arise from "parallax" due to inexact focussing.

In these experiments the displacement was so great that the slit moved entirely outside the field of light. To avoid this difficulty, the other parts of the apparatus, viz.: $ab$, the glass mirror, $d$, the cross-hairs, and $l$, the lens for viewing the image — were moved by the screw $w$ in the opposite direction past the slit; the distance moved, which is exactly equal to the displacement, is accurately read on the divided circle $c$.

The piece of glass $ab$ was partly silvered in order that when the mirror was at rest, the image of the slit would be seen from the unsilvered portion, while the *displaced* image, which is very much fainter, would be seen by reflection from the silvered part.

The annexed table gives the results of ten independent observations, made under difficulties and with apparatus adapted from the material found in the Laboratory of the Naval School:—

RESULTS OF OBSERVATIONS.

VELOCITY OF LIGHT IN AIR IN MILES PER SECOND.

| | |
|---|---|
| | 186,730 |
| | 188,820 |
| | 186,330 |
| | 185,330 |
| | 187,900 |
| | 184,500 |
| | 185,000 |
| | 186,770 |
| | 185,800 |
| | 187,900 |
| Mean — | 186,508 |

Formula, $V = \frac{4\pi r n D}{\delta}$

$V$ = velocity of light.
$r$ = radius of measurement.
$n$ = number turns per sec.
$D$ = twice dist. bet. mirrors.
$\delta$ = displacement.

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Their accordance with each other and with the generally accepted result justify the expectation of obtaining, with proper appliances and under more favorable conditions, the correct result within a few miles; and, I trust, justify the demand I have made on your time and attention.

In conclusion, I take this opportunity of tendering thanks to Mr. A. G. Heminway, of New York, for contributing $2,000 for the purpose of carrying out these experiments.

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## Outline of The Work done by the Fort Worth Solar Eclipse Party. By John K. Rees, of St. Louis, Mo.

This party was composed of Leonard Waldo, of Harvard College Observatory; R. W. Willson, of Harvard University; W. H. Pulsifer, of St. Louis; F. E. Seagrave, of Providence, and the writer.

Fort Worth had been chosen as the place of observation for several reasons:

1. Weather reports obtained from Colorado and Texas showed the chances for clear weather to be better in Texas.

2. The totality came to Fort Worth fifteen minutes later than it was seen at Las Animas, and, therefore, it seemed probable that we would be able to show any changes occurring in the corona, by comparing our photographs and sketches with those of the northern parties.

Mr. Willson left St. Louis July 8, to arrange for the exchange of longitude signals between St. Louis and Fort Worth, and to make the needed arrangements for the party's work.

On the night of July 15, an exchange of longitude signals was made, through the kindness of Col. R. W. Clowry. Mr. Waldo, at this end of the line, made use of a transit instrument set up in the observatory of the Washington University. Mr. Willson used a Stackpole sextant and subsequently a transit loaned by Brown University, Providence.

The rest of the party, except Mr. Pulsifer, reached Fort Worth

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on the 18th. Mr. Lomax, cashier of the City National Bank, generously placed his grounds, houses and horses at our disposal. The days before the eclipse were occupied with work on the adjustment of instruments and observations with sextant and transit for latitude and time. Mr. Willson had contrived a very steady sextant stand, which enabled Mr. Waldo to make observations on Polaris and Antares with great accuracy. Observers along the path of totality in Texas were requested to coöperate with our party; descriptive articles were written for the papers, and selections of Prof. Wm. Harkness' valuable instructions were printed and sent to each person desiring to act with us.

We readily established observers at Dallas, near Hearne, McKinney, near the limit of shadow, Allen and Cleburne. Some of these observers were furnished with stop-watches. A class of some twenty sketchers was organized, under the direction of Mr. Waldo, and were trained in sketching from pictures of the corona —allowing them 2 m. 30 s. to each sketch. The practical part of the photographic work was put in the hands of Mr. Alfred Freeman, of Dallas.

The instrumental equipment of the party was as follows: Mr. Waldo had under his care the photographic apparatus, including two cameras supplied with double refracting prisms, loaned by Dr. Litton, and arranged after the suggestion of Prof. E. C. Pickering, for photographing any effect produced by polarization. Mr. Waldo also used a small telescope for observing contacts. Mr. Willson used a beautiful three-inch telescope, kindly loaned the party by E. Ellery Anderson, of New York City. Mr. Seagrave had a five-inch Clark refractor, to which he adapted his own spectroscope of ten prisms by Browning. Mr. Pulsifer joined the party four days before the 29th, bringing with him his Clark four-inch telescope and two spectroscopes, one of which was fitted to a spare three-inch telescope belonging to Mr. Seagrave; the other was attached to Mr. Pulsifer's own instrument. I had in hand a five-inch Alvan Clark spectroscope, loaned by Smith and Porter, of Boston, to which I had attached a very fine spectroscope, of two prisms, and furnished with long collimator and long telescope.

Although the weather for two days before the eclipse promised badly, yet on the afternoon of the 29th the clouds broke only a few minutes before the first contact, and we had a fine view to nearly the last contact.

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In regard to the work done during the eclipse, Mr. Waldo reports as follows:—

1. He saw the diffraction shadows flitting over the ground at totality.

2. First contact, 3 h. 11 m. 29 sec.; duration of totality, 2 m. 28.7 sec.; fourth contact, 5 h. 19 m. 16 sec.

3. The photographs show detail, though moved, shown by the small prominences drawn out along the plate.

4. The corona was very different from any of the coronas drawn during preceding eclipses. The parts of it towards the North and South Poles of the heavens seemed pretty well defined, but the two ends which lay roughly in the ecliptic were very poorly defined, and probably extended much farther than the naked eye saw.

There was not visible at a glance any of the wonderful streamers portrayed during former total eclipses as radiating from the sun, but there were visible variations in the light from different parts of the corona, which gave the impression of unequal densities.

Mr. Willson, with the three-inch Secretan, with universal mounting, observed the first contact with terrestrial eye-piece, magnifying forty diameters, and noted the second contact with an inverting eye-piece of one hundred and fifty diameters. During the partial phase both these eye-pieces were used. For third contact and during totality a power of about thirty diameters was used, the field of view being about 1° diameter. In this eye-piece was a glass plate ruled by Prof. W. A. Rogers, of the Harvard observatory, with concentric circles about 6 minutes of arc apart, whose circumferences were divided into arcs of 45° each, by lines radiating from their common centre.

Mr. Willson was also provided with an ordinary Arago polariscope (loaned by Dr. Litton), and with one of Browning's direct vision spectroscopes to be used as occasion should offer. Observations were made of all the contacts. Mr. Willson, in his paper on the subject, says: "Just before the totality, I replaced the darker shade upon the telescope by a light reddish glass and caught up the chronometer beat, watching for the contact with my left eye, my right being bandaged. Totality having commenced, I removed the bandage from my right eye and examined the corona. My first glance caused me great surprise. The moon's disk appeared surrounded by a reddish glow (with a slight orange

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tinge), much brighter towards the moon's limb, of uniform texture in all parts of the circumference, but fading gradually to a well-defined surface about four or five minutes from the moon's limb. Neither chromosphere nor prominences were anywhere visible; this was my first, and I must say, disappointing impression. Just as the time-keeper called 2:15, however, I discovered that I had forgotten to remove my glass shade, which I immediately did, regretting that I had lost fifteen seconds of precious time—perhaps not lost, I think now, for my observation of the portion visible through the shade may prove to be of use, and seems to discriminate between the colors of different parts of the corona; for, as soon as I removed the shade, I became aware of other portions of the corona whose light was very nearly as intense as that near the moon's limb, but which were invisible through the shade, while the ring seen with the shade was not at any time distinctly seen without it. To my view the light of the corona varied somewhat in the different portions; and its outlines were very irregular, but well-defined."

Of the protuberances visible, only one was of good size, extending somewhat more than a minute and a half from the sun, and having somewhat of a sickle shape, the others were all small, but of a peculiar appearance. Mr. Seagrave reports that the thermometer fell 11° during totality. The first intimation of the moon's approach was given by Mr. Seagrave, who, intently observing with his spectroscope, called out, "Chromosphere gone!" Willson noted the contact six seconds later. Mr. Seagrave says he saw a continuous spectrum with one bright line 1,474, which was measured at the base. At the critical moment, Mr. Pulsifer's assistant, completely overcome by excitement, was unable to lend him any help. An untrained man stepping in at this juncture did what he could. Mr. Pulsifer intended to use both of his spectroscopes with his own four-inch telescope, but experiment showed that too much time would be lost in changing. The single prism spectroscope was adapted to the spare three-inch Clark telescope, which had a very shaky mounting and stand. The direct vision spectroscope was fitted to the fourth Clark telescope. For the reason above named, Mr. Pulsifer lost the spectroscopic contact. Removing his single prism spectroscope and adjusting his solar diagonal eye-piece, he observed the sun in partial eclipse; saw few faculæ; serrated edge of moon's limb very sharply defined; cusps

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clean cut; no brushes of light at cusps; granulations and faculæ extended close to moon's limb. The direct vision spectroscope was fitted to telescope sometime before totality. New prominence lines were looked for, but none found. About three seconds before totality the slit was placed tangential to that part of the sun's image where the second contact would occur, narrowed and focused till the spectrum came out beautifully clear. The field covered the *C* line. At last there came an instant when it seemed that the spectrum entirely vanished, then a flash, and the lines blazed out bright, the whole phenomena occupying so short a time that no estimation of its length could be formed. Mr. Pulsifer describes the phenomenon as follows: "I was startled to find the lines shortened at each end, and occupying not more than one-third or perhaps one-fourth of the width of the spectrum. I had never seen anything of the kind before, but was instantly reminded of Miller's maps of the ultra violet spectrum of platinum, but the lines were sharp, not nebulous, except that the ends seemed rounded off. It seemed to me that the *C* line did not shorten, or at least to such a degree as the other lines, and that it remained in sight for an instant after the other lines had disappeared." In regard to whether prominences and corona are visible before totality, Mr. Pulsifer says: "I can furnish some information on this point. I observed the eclipse of 1869 from my observatory in St. Louis, some twenty-five or thirty miles southwest of the line of totality. At that time I observed several prominences on the limb of the sun, just at the points where the limb of the moon exactly (or nearly so) covered the sun's limb. I made a drawing at the time, but it has been mislaid or destroyed. The corona also was visible."

My own observations, during the partial phases, showed the moon's limb very irregular, and, toward the centre, affected by a slight undulation. This undulation was not communicated to the cusps, which were very sharply cut. In the extreme points, both before and after totality, dots of light were observed, of the same color as the faculæ. During totality, the spectrum of the corona, pointing the telescope to four points of the corona, was always continuous, and the Fraunhofer lines *C* and *D* were plainly visible.

The light of the "wings" of the corona was varied, giving, to my eye, the appearance of parallel rays, and not radial. These



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wings were in the line of the ecliptic, and the rays were also parallel to the ecliptic.

The darkness during totality was not so great as I expected. I was well able to read fine divisions on my slit screw-head without any artificial light.

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Col. Lockett, of Tennessee, made the best sketch the party had, and a copy in oil of his sketch was exhibited by Prof. Rees. Prof. Barker showed the *Graphic* copy of Draper's photograph of the corona, and compared it with the telescopic view by Mr. Willson.

The liberality of a few gentlemen of St. Louis made it possible for the party to go to Fort Worth well equipped. The party expect to publish shortly the results of their observations and therefore the above account is brief.

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## An Improved Method of Ringing a Bell in an Exhausted Receiver. By Thos. R. Baker, of Millersville, Pa.

[ABSTRACT.]

A vibrating electric bell is so combined with the battery by which it is rung, that both can be placed under the receiver. The apparatus is represented in the accompanying figure.

The lid of the box containing the battery cell supports the bell, the wires passing out through small holes. The box, on the outside, is $3\frac{1}{2}$ inches square and $3\frac{1}{4}$ inches deep, and the bell is about $6\frac{1}{2}$ inches above the top of the lid. The apparatus may, however, be made to suit the receiver.

The bell may be made to ring continuously during the exhaustion of the air, thus showing a gradual decrease in the intensity of sound, or it may be rung at any time during the exhaustion by a simple contrivance for closing the circuit. A W-shaped tube open at both ends, and with a copper wire entering the end *a*, and extending to the bend *c*, is fixed upright upon the lid. A small quantity of air is confined in the middle bend by filling the outer

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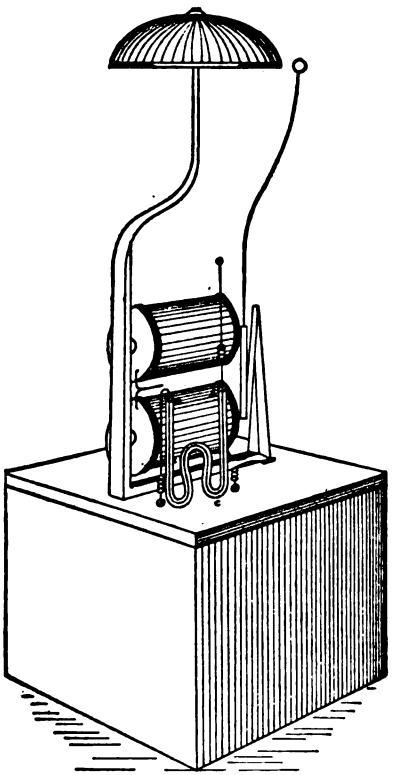
bends with mercury. The battery wires are fixed in a vertical position, and, when the lid is placed on the box, the end of one of them presses into a small spiral forming the outer termination of the tube wire, and the end of the other into a coiled end of a wire leading to the post against which the armature vibrates. In the end *b* of the tube is a copper wire capable of moving tightly

Fig. 1.



upward and downward through a spiral forming one end of the magnet wire. The other end of the magnet wire connects with the spring supporting the armature, as in ordinary electric bells.

When the apparatus is placed under the receiver, with the lower end of the movable wire above the mercury, and the pump worked, the expansion of the air confined in the middle bend will force the mercury up to the wire, thus closing the circuit and causing the

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bell to ring. The ringing may be started at any time by properly adjusting the movable wire.

Daniel's cell may be conveniently used to ring the bell, the copper cup being made to fit the box. Two small porous cups may be used instead of one. This cell has sufficient force to ring the bell vigorously. The apparatus should rest upon a thick layer of loose cotton, or cushion of felt, to prevent the sound from being conveyed through the metallic parts of the pump.

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ON THE CONSTRUCTION OF A SENSITIVE WIND-VANE. By J. W. OSBORNE, of Washington, D. C.

IN a paper submitted to this section at the Nashville meeting, I described a light and sensitive form of wind-vane, the arm of which was 5 feet 4 inches long, and its weight 16 ounces; counterpoised by a piece of lead at a distance of 13 inches from the centre of motion, weighing $2\frac{3}{4}$ pounds.

This proved to be a very serviceable vane, and is still in constant use; but it was found that its inertia, and the momentum which it acquired when moved by the wind — light as it was compared with ordinary instruments of the kind — took from its sensitiveness, and gave rise to oscillations which were not indicative of the veering of the wind alone. A perfect wind-vane should instantly respond to the slightest change in the direction of the wind, and should remain stationary when it has made the necessary angular movement; in other words, it should continue at all times parallel to the current in which it lies, be the same strong or weak.

How nearly this perfection was achieved in the instrument constructed in the present year, the following description will serve to show.

This wind-vane was 7 feet long, while its weight was only three ounces (1310 grains), and as the method of its suspension made a counterpoise unnecessary, the evil effects of momentum were still further diminished — practically, in fact, they did not

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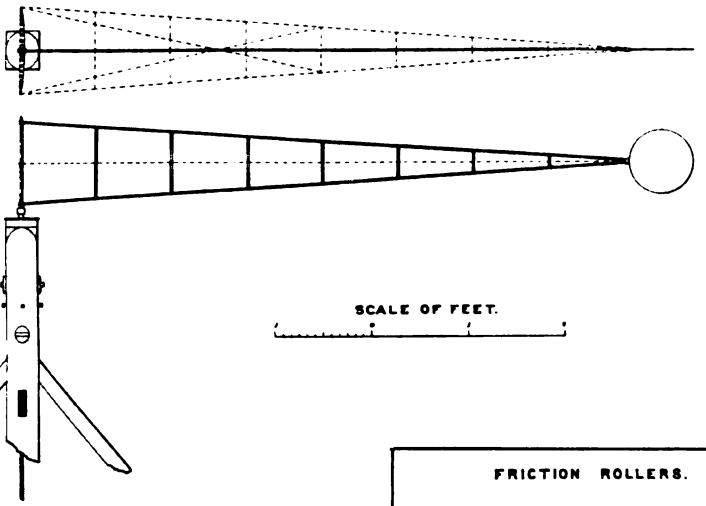
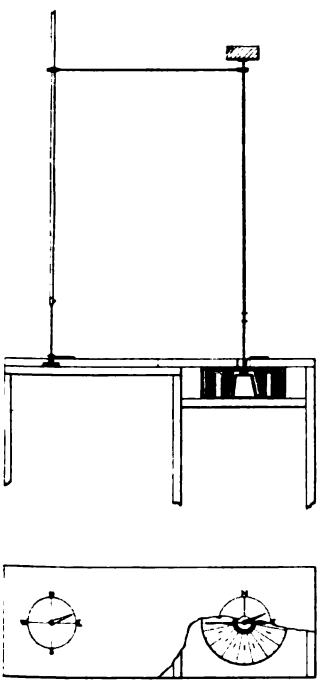
# CONSTRUCTION OF A SENSITIVE WIND VANE.



SCALE OF FEET.



*N.B. All dotted lines indicate wire.*

## FRICTION ROLLERS.

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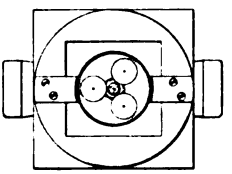

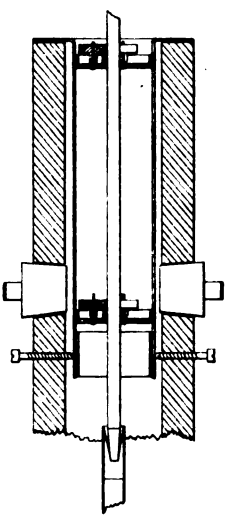

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exist. The material used was the thinnest sheet brass obtainable, rolled hard for this particular purpose, weighing 520 grains per square foot, and having a thickness of 0·0017 inches. This was made into tubes $\frac{5}{16}$ inches diameter, and of these tubes the arm was constructed as shown in the drawing, at the end of which a flat, circular disk was attached to receive the impulse given by the wind. The disk 7 inches in diameter, equal to $\frac{1}{4}$ square foot of surface, was made of two sheets of tinsel (a material about half the weight of the sheet brass—243 grains per square foot), strained tight and soldered to the sides of a ring of steel wire. This disk was perfectly stiff and flat, and weighed only $\frac{3}{4}$ ounces. To give rigidity to the vane laterally, two light arms constructed of piano-wire projected from the spindle, one on each side, and from their extremities to the junction of the disk with the long tubes, fine brass wires were strained. These were again soldered to cross wires attached to the upright tubes of the truss in a manner which the drawing will make intelligible, and finally, two diagonal wires were introduced to stiffen the middle section of the arm laterally. All these wires must be drawn up equally on both sides—an operation which requires some tact and judgment—before soldering the places where they cross; but when this is successfully accomplished the whole structure will be found to possess remarkable elasticity and strength.

The weight of this vane (3 ounces), includes all except the upright spindle, which must be strong enough to resist the up and down strain put on it when the wind affects the vane in that way, which is the most dangerous stress it has to withstand. The spindle was $\frac{3}{8}$ inches in diameter, and ran between two groups, of three friction-rollers each; one as near the vane as possible, and one 8 inches lower down. The friction-rollers run on small polished steel studs, standing, each set, in a little tray of oil which keeps them thoroughly lubricated for many months together. The spindle is connected at its lower end with the rod (12 feet long), passing through the roof in such a way that while there is no shake in a rotary direction, there is sufficient play to prevent any strain being thrown upon the spindle when it oscillates. The "rod" consists of a tube of thin sheet tin. At its lower end it carries a hardened steel point which runs in a polished cavity in a piece of glass. On this point rests the whole weight of the instrument, amounting to 19 ounces.

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This vane was supported 8 feet above a flat roof on Capitol Hill, Washington, 55 feet above the ground and 123 above the Potomac, side by side with that described in the paper read last year before the Association. The rod passes through a timber trunk well staid upon the roof, and the friction-rollers — the lower set being adjustable — are so fixed that the spindle is perfectly vertical. A small collar and cap which are not in contact prevent entrance of water where the spindle enters the trunk.

It will be seen that in the construction of this vane every effort was made to reduce friction and inertia. It was carefully set up in a closed room before putting it on the roof, and the mean of many experiments showed that $1\frac{1}{4}$ grains lateral pressure, perpendicular to the center of the flat disk, was sufficient to start, and keep it in motion against stationary air. It may be also interesting to note that, although $\frac{1}{3}$ longer than the other vane, $10\frac{1}{2}$ ounces at 13 inches distance from the spindle was sufficient to counterpoise it, whereas the other required 44 ounces as already stated.

The behavior of the wind-vane I have described was most admirable; a simple inspection of its oscillations in light or heavy winds, convincing any one of the truthfulness of its indications, and extreme sensitiveness. As an experimental vane it could hardly be surpassed. By its use a classification of the winds in reference to the peculiar character of the oscillations accompanying them was possible, and offered matter for research of great interest. It was found that the angular scope of the oscillations, their frequency and regularity, differed greatly, even with winds from the same quarter and of the same strength.

In the case of ordinary vanes, such as those used by the U. S. Signal Service, weighing from 30 to 40 pounds and more, running on collars of large diameter, difficult or impossible to lubricate in a satisfactory way, the lighter winds are practically lost, for they produce a movement only when the veering is considerable, and then of disproportionate extent; while of those which blow with greater force, all the finer characteristics are eliminated, and the oscillations — influenced by momentum — do not represent the actual veering of such winds at all.

It is true, however, that for practical purposes, when the main direction of the wind is all that interests the observer, it is not unfrequently difficult to determine what that direction is from

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watching the erratic and unceasing movements of the pointer swinging over several points, when the vane is highly sensitive; and some experience and judgment are necessary, if accuracy in this respect is required.

It is obviously unphilosophical to overcome this difficulty by adding to the inertia of the vane, and by retarding it with an overwhelming amount of friction which varies from day to day, and which is vastly greater when the vane begins to move, than when it is in motion. The true plan it appears to me, is to let the vane give expression to all the facts, and then deal with the record so as to obtain a mean direction. The mean of the angular movements of the pointer alone, will not give the desired information, because sudden impulses very frequently throw the pointer repeatedly to great distances on one side of the main direction, which are compensated for only by small, slow and deliberate oscillations on the other, which latter, though they make less show, are, by reason of their persistency, much nearer the truth. It would seem, therefore, that duration as well as quantity has to be taken into acconnt.

The solution of this problem by mechanical means was attempted in the following way. A vertical shaft of small diameter, consisting of a thin brass tube terminating in a steel point, runs on a glass bearing. It carries above a small horizontal pulley, which is driven by a very small belt made of thin brass, from a pulley of exactly equal size on the rod of the wind-vane.

The lower extremity of the shaft, consisting of a steel wire $\frac{1}{10}$ inches in diameter, carries a free collar of brass, to which a pointer is attached, and also two or more horizontal arms from which dependent blades or vanes of thin metal hang in an annular vessel containing oil. The collar is connected with a long and delicate spiral spring (360 coils) encircling the vertical shaft without touching it, the upper end of which has also a small collar sliding upon the shaft, and made fast to it at any desirable place by a set-screw. When in adjustment, the collar below with its vanes and pointer is hanging upon the spring. When the wind-vane oscillates, the little vertical shaft oscillates synchronously with it; but the lower collar with its pointer, and the blades immersed in the oil, receives the impulses through the spring as a medium; and as the resistance in the oil increases at least as rapidly as the square of the velocity, the quick spasmodic movements of the wind-vane are eliminated

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in part, and those of a more deliberate and permanent character proportionally represented. In practice it will be found that the veering of the wind, as indicated by the pointer of this attachment, is relatively slow and steady, covering small arcs only; and the angle thus included is itself indicative of the veering in the main direction. Thus upon the same table we have two pointers, one representing the local condition of things upon the roof, and the other—approximately at least—the changes in the great current sweeping over the place of observation. Of course the insensitiveness of this instrument to erratic movements is in perfect control as it depends upon the stiffness of the spring; the diameter of the oil vessel; its depth, and the viscosity of the fluid employed. With regard to the fluid, I have found a heavy kind of petroleum known as "mineral sperm" answer very well. Should the temperature of the room fall very low in winter, this substance becomes viscid, and it is probable that a solution of salt, or of calcium chloride in water, covered perhaps with a film of petroleum, will prove serviceable. It should be added, that it is quite necessary to attach a number of stationary pieces of sheet brass to the sides and bottom of the oil vessel as shown in the drawing, for the purpose of checking any tendency to a vortical motion in the fluid.

It is plain that this instrument might be made part of the wind-vane, and in some cases this would be desirable; but in general, for purposes of investigation, it will be found better to make it a separate attachment as here described. In whichever way the principle is applied it has great advantages over an attempt to gain similar results by impeding the motions of the vane itself through the direct introduction of resistance. Such methods throw a strain upon the vane which may be very severe in stormy weather, and demand a proportionally strong construction. By the use of the spring the only force required is that necessary to coil or uncoil it to the extent of one revolution at most; a strain which is quite inappreciable.

Returning to the wind-vane. It should not be supposed that, for the usual meteorological record, I advocate the use of an instrument of the extreme delicacy of that which forms the subject of this paper. The material employed should be much stronger and stiffer, as, for instance, thin sheet tin. The friction-rollers should be retained, the method of lubricating them, and the glass bear-

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ing below; but a counterpoise should undoubtedly be added. This is desirable, because it is a matter of considerable difficulty to adjust the friction-rollers so that the spindle shall be truly perpendicular, and the difficulty increases the higher the vane is placed above the roof. Even when this is done, if there be any swaying of the trunk carrying the vane, the latter will have a tendency to fall to the lower side. The vane I have described withstood many violent storms and squalls, some of them doing much damage to property in the vicinity; but it was finally destroyed during the exceedingly violent gale which accompanied a thunderstorm on the 10th of August of this year. As one, at least, of the storms it had been exposed to seemed quite as severe as this one, it is very probable that the tubes received a blow from one of the flying branches or twigs with which the air was filled, and in which way it had suffered slight injury once before. In this connection it may be also interesting to state that the thin brass had received a large number of indentations from hail-stones of the size of large peas, which fell on two occasions.

In constructing wind-vanes it is very usual to provide them with two diverging blades, evidently with the intention of receiving the impact of the wind under favorable conditions. Apart from the fact that these blades, when they start from the spindle are nearly useless for a great part of their length, and only add weight, it is a mistake to suppose that they increase sensitiveness, if, by that term, we understand the obedience of the vane to the first slight change in the direction of the wind; as it can be easily shown that the smaller the angle which the blades make with each other, the greater is the difference of the lateral resultants acting upon them, for all those changes in direction of the current which are small enough to affect both blades at once. Any vane will move when the wind has veered round considerably; gaining power under such circumstances is a matter of indifference; what is wanted is the application of the maximum force attainable at the first moment of change. But there are other good reasons why the whole of the arm should be kept in one plane. In a vane with divergent blades, inequalities in the *pressure* of the wind affecting them will produce slight oscillations which may be confounded with those due to changes in direction, and generally an unstable condition will result, which increases in proportion to the angle that the blades make with each other. In the case of a light vane, the vibratory

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movement resulting from this cause and the greatly increased tentional strain which the instrument has to withstand in heavy winds from the increased resistance due to the divergence, tries its strength very seriously, and adds to lateral friction quite unnecessarily.

As to the length a wind-vane ought to have for practical meteorological work; the experiments made so far tend to show that great length is quite unnecessary; especially if an attachment like that I have described is employed to modify and control irregular oscillations, due to local causes. But, for investigations into the nature of the currents lying upon the surface; the part played by the earth itself in retarding and disturbing them; and the form of the path traversed by atmospheric masses; for these purposes, vanes of widely different lengths will be found to give most instructive results.

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WIND-VANE ROTATIONS. By J. W. OSBORNE, of Washington, D. C.

IN a paper last year before the Association, two ways were described by which the preponderating revolutions could be counted which a wind-vane makes in either direction. On the present occasion I wish to bring to the notice of the section an improvement in one of those methods, which for simplicity and accuracy leaves nothing to be desired.

The rod from the vane upon the roof terminates below in a vertical steel spindle rather less than one-tenth of an inch in diameter. The lower end of this is pointed and hardened, and runs in a glass bearing supported immediately below the table on which the thirty-two points of the compass are marked, and through which the spindle passes. A horizontal index, attached by a set screw to the spindle, traverses these points, and indicates the position of the vane above.

Outside this circle is another, the circumference of which, starting from any convenient place, as zero, is divided into tenths and hundredths, and numbered both ways. Over this circle traverses another index, above that already described, and some

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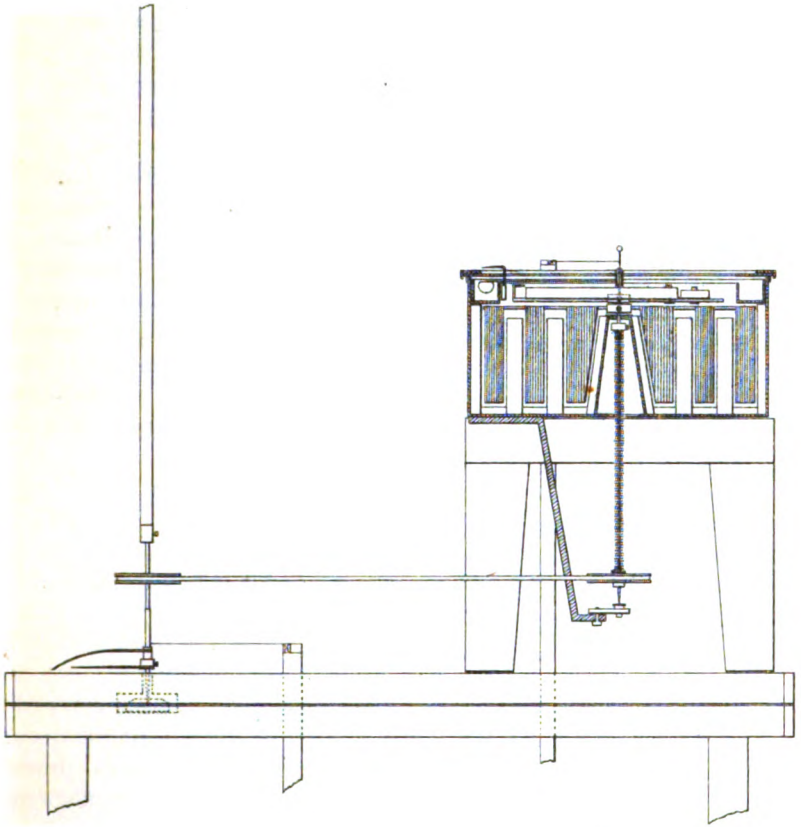
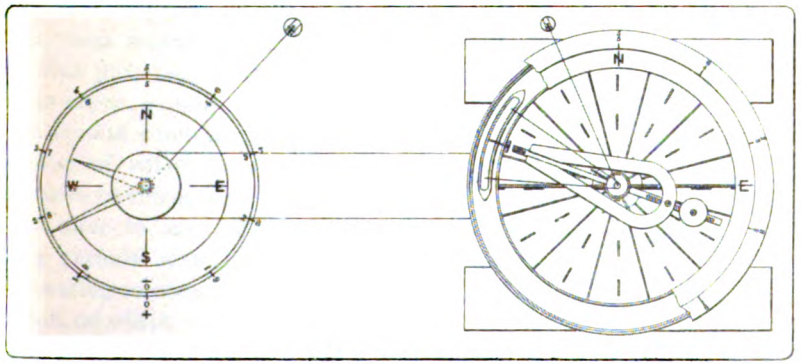
# WIND - VANE ROTATIONS.






SCALE OF INCHES.

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what longer than it. This is attached to a sleeve of thin metal on the steel spindle, the friction between them being sufficient to move the index. To one side of the sleeve, which extends some way up the spindle, a fine thread is made fast, extending radially from it to a little free pulley over which it passes, carrying a weight of a few grains at its pendant end. This thread is arranged so that none of it is on the sleeve when the pointer is at zero.

Starting in this way the thread will be wound upon the sleeve according as the spindle revolves. After the interval has elapsed, for which it is desired to count the preponderating rotations, an inspection of the thread will show in which direction it was wound up, and therefore whether the movement was positive or negative. The position of the upper pointer, then, gives upon the outer circle, the fractions of a revolution made; that position corresponding of course to a certain reading of the direction of the wind which has been fixed upon—say N. E. The upper pointer is now laid hold of, and moved round backwards till it comes to zero. If, then, there is more thread upon the sleeve, the turning is continued till all the thread has been unwound. The number of revolutions required to do this, gives the whole number, which precedes the fraction already obtained. By holding the upper pointer at zero till the moment when the lower pointer, oscillating with the wind, passes the reading fixed upon (N. E. in the supposed case), and then releasing it, the instrument is set for the next interval. In practice this will be found a simple and efficient method by which to count the revolutions with accuracy, either for long or short periods, and one which does not interfere with the free motion of the vane itself.

By such means a long series of observations was made, using for that purpose the exceedingly light vane described in the previous paper, as well as the heavier one which was also referred to. The results confirm and extend those of last year. The total number of rotations is very great, of which those moving in a negative or retrograde direction, are in large excess, rather more than three to one. These preponderating negative rotations for long periods may be regarded (for speculative purposes), as amounting to about 1·5 daily; but this statement requires qualification, of which more hereafter.

The tendency to rotate is not always present, it recurs at longer or shorter intervals, so that the record shows the rotations congre-

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gated together in groups. It also shows that at any one time, the prevailing character of such a group is either positive or negative; more rarely of a mixed nature. It has been found impossible to connect the revolutions themselves, or any of the peculiarities attending them, with winds blowing from particular quarters of the compass; they appear to be both present and absent with all. But the *force* with which the wind blows does seem to influence the results in certain ways, and this leads to a consideration of the comparative behavior of the two vanes which were used in this investigation. In the preceding paper both these instruments have been sufficiently described. They were set up at a distance of seven feet six inches apart, the lighter vane being one and one-half feet above the heavier one. With the help of a simple mechanical contrivance the index and counting apparatus of the latter were transferred to the table of the former, so that the two pointers worked side by side, and admitted of easy comparison.

By carefully watching both vanes, it soon became manifest that the rotations which each made were very discrepant in regard to number; still less were they simultaneous; and occasionally they differed in direction.

In light winds, the lighter and longer vane made the larger number in a given time; in strong winds the same was true of the heavier vane. This was obviously due to the greater sensitiveness of the one, and the greater inertia of the other. But on days when revolutions were abundant with both (rising sometimes to as many as fourteen in twenty-four hours), it very frequently happened that when one revolved, the other did not do so; in fact it was exceptional when they both went round the circle together, and the cases were by no means rare, in which, while one moved in a positive, the other did so in a negative direction, though not perhaps at the same moment.

These facts are only true of relatively rapid rotations; those in which after making a revolution the vane returns again to a position where it oscillates about the main direction of the wind. In fact they may be themselves regarded as large oscillations, exceeding 180° from the main direction; in which the impulse, that under ordinary circumstances would carry the pointer back over its course, takes it round the shorter way instead. These rapid rotations vary in the time required to complete them, from fifteen seconds, to about 150; perhaps forty seconds may be taken as

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their average duration with a vane seven feet long. The onward motion of the pointer is often slow and steady; but more frequently there are pauses, and sometimes short retrograde movements. In considering this subject, it is important to dismiss from the mind any conception of a really sensitive vane, as *swinging* about its axis. Where the arm is at any moment, it has been *put*, and there it will remain till carried on again, and whenever it continues in motion slow or fast, we may be certain that the wind is following it all the time, and doing all the work. Experience has shown that this, for all practical purposes, is equally true of a vane much heavier than the delicate one described in my previous paper.

All these phenomena are to be found in the incessant *oscillations* of a vane which is sufficiently sensitive. We find the amplitude of such oscillations varies very greatly at different times without any assignable cause, and when they are large, we find in them the same pauses and short retrograde movements. It is clear that the angle they include, and the rapidity with which they are made, are influenced (as well as the rotations) by the length, weight and friction of the vane, and it is also apparent that they are in part determined by the configuration of the surface, and by irregular projections upon the roof, or in the immediate vicinity; but behind these causes the fact remains that certain winds seem to carry with them the tendency to deviate from a direct course, so that varieties of oscillation are after all due to inherent characteristics of the wind. To speak of the appearances I have been describing as simply of local origin, is not a satisfactory explanation, because the local conditions are always present unchanged, while the phenomena themselves vary greatly. Such an explanation, moreover, leaves altogether untouched the remarkable predominance of negative rotations.

In this connection, the modifying effect of the force or rapidity of the wind should be considered. The observations show, as before stated, that as the wind increases in velocity, the rotations of the lighter vane generally decrease in number, while those of the heavier one, especially in gusty, blustering weather, are often relatively increased, a circumstance evidently due to its momentum, and one which accounts for its larger total number of rotations, particularly at certain seasons of the year. The lighter vane gives the true state of things for strong as well as gentle winds; and the

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reason why its oscillations are usually smaller, and its rotations fewer in windy weather, is because the onward motion is so quick, that an impulse from one side is immediately succeeded by one from the other, and we obtain in consequence, short, quick, movements of the vane. But the characteristics of such winds differ just as widely as those of less rapid currents, and the fact still remains that the origin of these differences is referable essentially to the wind itself.

That remarkable deviations, both lateral and vertical, take place in the main direction, no person who has had an opportunity of studying the phenomena, can for a moment doubt. How, or where the force is applied which produces these deflections, are questions of great difficulty, which, like many others in physical meteorology, can neither be fully investigated, nor solved, till means are provided for making observations at great heights. But it may not be out of place to state here, without entering the speculative field, that the rotations when closely watched in fine, still weather, have frequently suggested the rise (or it may be fall) of masses of air, and the following after of lateral currents coming successively from all points of the compass, and producing thereby one turn of an imaginary spiral. Be this as it may, it is quite safe to assert that a study of the behavior of sensitive wind-vanes tends to give reality to the wonderful mixing-process affecting the atmosphere at the earth's surface, and to impress us with the importance of including it in the consideration of aerial phenomena generally.

Having especially referred in the foregoing to local rotations of the wind, I now pass to those of the main current. In the preceding paper, a mechanical method was described, which, dealing with all the oscillations made by a sensitive vane, eliminated those due to local conditions, in a greater or less degree, according to their actual importance; so that the remaining oscillations represented very fairly the changes in the main direction. The function of the instrument now to be described, is analogous, inasmuch as it is designed to eliminate those rotations affected by local and subjective causes, and to count only such as are true revolutions of the leading current; doing this without interfering with the free movements of the vane, even in the lightest winds. In connection with the accompanying plate, and what has been said in the previous paper, a few words will make its construction intelligible.

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An oil vessel, similar to that already described, is provided with a steel spindle of small diameter, which extends below the bottom of the vessel, and not above it as in the other attachment. It is pointed and hardened at its lower end and runs on agate. Its upper bearing is in a thin plate of glass, through which a hole is bored, the sides of which are carefully polished. To the spindle two arms carrying depending blades hanging in the oil are attached, and a long spiral spring of fine steel wire (Zitter wire) is fixed, with a collar and set-screw to its upper part, immediately below the glass bearing. The lower end of this spring has dependent from it a free pulley, which has two thin glass bearings in it, separated three-eighths of an inch, so as to reduce friction to a minimum and keep it horizontal. This pulley is driven by a metallic belt one-eighth of an inch wide, from one of exactly equal size fixed on the rod of the wind-vane. So far, the construction is but a modification of that already used for damping the oscillations; but, in place of the pointer, an arm carrying a good horse-shoe magnet is attached horizontally to the upper part of the spindle. The magnet is adjustable, so that the distance of its poles from the centre of motion admits of modification, and it can also be counterpoised in every position. In the plane of the magnet, and concentric with the spindle, a small, annular trough is placed. This is partly filled with oil on which there lies a float of very thin metal. The float is connected by two fine wires with a light cap of brass resting upon the top of the steel spindle, which is delicately pointed to receive it, and admit of its free motion without friction. The cap terminates above in a very slender steel wire about two one-hundredths of an inch in diameter, encircled by a minute tube of tinsel foil, from one side of which an exceedingly fine silk thread extends, and, hanging over a little free pulley, carries a weight amounting to a fraction of a grain, just sufficient to draw it straight. Above the annular trough is a flat plate also in the form of a ring, on which the thirty-two points are marked, and upon this lies another of larger diameter, divided into hundredths of its circumference which is traversed by a pointer from the arm bearing the magnet. This second ring is movable on that below it, and a number of studs projecting from its lower surface keep it central in every position. Finally, the float carries a small piece of iron, which forms an anchor for the magnet, though never near enough to touch it.

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When in use the whole instrument should be covered by a cylinder provided with a movable glass top to exclude dust and currents of air. This is not shown in the plate.

When connected with the vane, the free pulley, on the steel spindle of this arrangement, oscillates synchronously with it. The force so applied is communicated through the spring to the spindle, and the result is a slow and steady oscillation of the magnet over small arcs, in a horizontal plane. Under these circumstances the float follows the magnet, lagging a very little behind. When a rotation of the main current takes place, always more or less gradual in its accomplishment, the float still keeps with the magnet, and the silk fibre is wound up upon the foil covering the wire projecting from the cap, and its movement is therefore counted. On the contrary, when the revolution of the vane is relatively rapid, the magnet starts at once upon a journey round the circle with an accelerating velocity; it leaves the float behind, and takes it up again after the spindle has once turned upon its axis. Such a rotation is obviously not counted. When the period fixed upon has elapsed the counting of the fractions is done upon the movable ring divided into hundredths, the zero on which is then moved to the position of the pointer, ready for the next interval; the whole number and sign, is given by the thread attached to the cap, which is then unwound and made to take its radial position from the cap by turning the little foil sleeve upon the projecting wire.

It may be added, that the horse-shoe form of the magnet is desirable on account of its greater power; and also, because it has no directing polar tendency, which, with so delicate a suspension, would seriously affect the result. It is necessary to be very particular in supporting the whole instrument, so that the spindle is quite perpendicular, otherwise the arm carrying the magnet must be counterpoised with very great accuracy, which is not easily done. The magnet should be strong enough to admit of being drawn back some distance from the anchor, to the end that the diminution of the force moving the float should be less rapid, and act at longer angular distances, without being so strong as to take the float with it under all circumstances.

The construction of this instrument is too recent to admit of anything being said of results furnished by it. It is evident that by simply retarding the movements of a strong vane sufficiently by friction, or impeding its motion in any analogous way, rotations

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due to local causes could be hindered; but in doing so, very important revolutions of the lighter winds would certainly be lost.

An exceedingly long vane (how long it is impossible to say), would no doubt give valuable results; but such, when sensitive, are difficult to construct, for the leverage in an up and down direction becomes considerable, and it requires great strength to resist the impulses so received, both in the body of the vane and its supports. A shorter vane, isolated at a great elevation, would doubtless give results of the most unexceptionable kind.

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## On the Importance of Meteorological Observations in Vertical Section of the Atmosphere, with the Suggestion of Means for their Systematic Accomplishment. By J. W. Osborne, of Washington, D. C.

Some years ago Professor Balfour Stewart pointed out, with convincing clearness, the importance of studying meteorology with broader aims than those which very frequently guide inquiries in that branch of natural science. He distinguished between what he called physical and climatic meteorology, and showed that comparatively little is done for the former with any distinct purpose. The labor which has been, and is still expended in reading and recording instruments, is very great; but only a fraction of the results obtained have proved of value in extending our knowledge of the laws which govern our atmosphere generally; as a part of our planet,—and in its cosmical relations.

Meteorological observations for climatic purposes,[1] when widely extended, and made with care and uniformity, do undoubtedly furnish valuable material on which to base comprehensive theories. In fact they may be said to furnish the only available material, if we leave out of consideration for the present, recent discoveries

[1] I have elsewhere endeavored to show that *climate* is essentially subjective, and that our meteorological observations which ignore the functional phenomena of living organisms, are statements of the weather and its changes, not of the climate of the place where such observations are made.

The *purpose* may be climatic, but the result is not.



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in solar physics, and in the influence which periodic changes upon the sun's surface appear to exert upon our atmosphere. The multiplication of stations and observations will not of themselves advance the higher developments of the science. As a matter of fact we know that large quantities of undigested material may exist without commensurate results. It is only when such records are properly reduced and discussed that real progress is possible. But without pursuing this subject further, the inquiry seems pertinent, whether observational methods might not be adopted better calculated to promote a knowledge of physical meteorology than those on which we now depend. Is it not possible to make our observations, as well as our discussion of them afterwards, with special reference to the ends in view?

We are dealing with a great aerial ocean many miles thick, revolving with the earth. We want to understand the action of the solar heat upon it: how that heat is converted into mass-motion. We wish to acquire a knowledge of the currents which pervade it, their direction, and interference one with another: where and how the stupendous forces are applied that move them: the influence of evaporation and of condensation in modifying other phenomena: the part played by atmospheric electricity. These and other questions equally intricate present themselves for solution, and make us realize our ignorance.

If we now consider what *is* actually done to investigate the phenomena and solve the problems of meteorology, we shall find that all the observations upon which so much labor and care are expended, are made upon the bottom of this aerial ocean. In the stratum lying in contact with the earth, affected directly by its irregular form, by its temperature, and by its moisture; we make our researches, and give to the answers we receive the widest application,—not always, it is true, exactly as we receive them, but modified as we think they ought to be modified in the regions above our heads.

As long as the aim of the meteorologist is to accumulate facts and make generalizations bearing upon animal and vegetable life, this course would appear to have some justification, for organic life is chiefly distributed over the surface upon which he works; but when his researches have a wider scope, when he seeks to connect our atmosphere with the universe beyond, and attempts to grasp its function in the great physical chain; then, it is surely

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unphilosophical for him to confine his investigations to one position, and to one small part of its immense mass, if, by any possibility they can be extended.

The importance of observations at great elevations has been felt and urged by many persons. Mr. Glaisher, above all others, by his splendid series of balloon ascensions, demonstrated his faith in their value. Stations have also been established on mountain summits, as on Mount Washington and Pike's Peak. But, while all must admit the great value of such efforts, and recognize, indeed, how indispensable they are, it seems open to question whether they can ever be made to give the information we most want in a satisfactory and economical way. In free balloons, the rapid change in altitude and in position horizontally, disconnects the observations possible in them, from any made below, and the record becomes that of a journey extending, it may be, many miles. In anchored balloons, the most we can expect are observations in fair weather, with the certainty of learning nothing when the wind is even moderately strong, and the further certainty of losing the whole apparatus very frequently. A station established on the top of a mountain is elevated, it is true, many feet above the sea, and systematic work is there possible; but it is still on the earth and in the earth-current which follows more or less accurately the contour of the surface, and which is seriously affected by the solid material it meets with, as is shown by the sudden changes in the weather of mountain regions.

What we want, it appears to me, is not only a knowledge of phenomena at high elevations, but also of the successive steps which lead up to the condition of things above, systematically and accurately determined by simultaneous observations on a vertical line.

If we accept this definition of the problem, I believe any one who gives it serious thought will come to the conclusion that it can only be solved by the erection of a tower especially designed for meteorological work. I have myself endeavored to suggest a suitable form of tower for this purpose, and have even made a model, part of which is before the section; but I am glad to have it in my power to exhibit a design of much greater importance, made for me by the well-known engineers and bridge-builders, Clarke, Reeves & Co., of Phœnixville, Pennsylvania, which is illustrated by the accompanying drawing. To these gentlemen I

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am much indebted for the trouble they have taken, and for earnest consideration of the necessities of the case.

This tower is constructed entirely of wrought iron. It i feet high, and is pyramidical in form. It consists essentia four Phœnix columns, connected by struts and tie-rods, an closes within it a parallel way for an elevator, consisting al four columns similarly braced. It is designed to contain the possible material consistent with strength, so that its mass practically have no effect upon the instruments, and offer but resistance to the wind. It is calculated to resist a storm of miles per hour with perfect safety. At every 100 feet a gallery is constructed, from which the instruments are read on which a suitable protection is provided for the self-regist apparatus, so that it can be examined and changed in all weat It will be seen that the proposed arrangement gives six stat counting the ground as one. Perhaps a subdivision of the 100 feet may be found desirable, adding a station at thirty-f and one at sixty-six feet for thermometers and hygrometers, to analyze more thoroughly the earth-stratum, where the rapid changes in temperature and humidity may be expected.

The approximate cost of such a tower is $35,000. With re to its height I may say, that, although it would no doubt b vantageous to have one much higher, it seems wise, in the instance, to be content with a moderate elevation; experience then show whether a more costly structure will give commensi advantages. Admirable results can certainly be achieved 500 feet, and a few years work will throw more light upo subject than can be gained by any amount of *a priori* reaso The point to be aimed at is to construct the first tower of su height, that the upper stations shall be out of the perturbed c stratum. This it is believed will be the case if a suitable loc is chosen for the erection of one like that described.

As explained already it is with reference to physical meteoro that results may be looked for from observations made in manner proposed. On weather predictions and question climate, the knowledge acquired would have an indirect e which need not be discussed at present. Bearing this in mi will be seen that such observations are not identified with a ticular locality, as are those made in the ordinary way, and hope may therefore be entertained, that from a single phy

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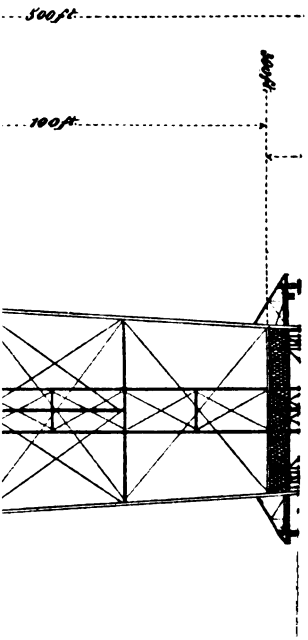


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observatory of this kind substantial results of wide and general application may be obtained. That hourly, or continuous observations of all the meteorological elements at six stations, on a vertical section of the atmosphere 500 feet high, must add greatly to our knowledge, hardly admits of doubt. It would extend the length of this paper too much to refer, with any attempt at completeness, to the several questions likely to be advanced by such a record after it has been fully discussed, but a few words may be said of some of them.

The source from which the atmosphere derives its vaporous constituent is the surface of earth and ocean exposed to it. If the total rainfall represents the total evaporation from that surface, we can form some conception of the quantity of water which passes through the lower stratum to be diffused throughout the whole mass of air above. If, then, the absolute quantity of watery vapor which the air contains at any particular time and place is to be used as an important aid to a knowledge of atmospheric movements and other phenomena,—it is surely wrong to make the determination a few feet from the ground in the thin stratum which acts as a carrier of moisture for all that is above it. Such a course it appears to me would only be justifiable, if it were actually impossible to make the observations at such a height as would enable some reliable approximation of the humidity of the aerial current throughout its thickness to be arrived at. That it is not impossible is plain; it is simply difficult; and if it can be shown that the acquisition of such knowledge is a condition of success, the sooner that difficulty is attacked and overcome the more rapid will be our advance in a department of scientific research which perhaps, more than any other, affects our immediate well-being. To some extent the effect of elevation above the ground on determinations of temperature and moisture, has been investigated by meteorologists; though very few of them have used scaffolding in the open field in the manner adopted by Professor Wild, of Pulkowa Observatory, and Doctor Hamberg, of Upsala, both of whom obtained valuable results. But the altitudes reached have never been sufficient, because they did not extend beyond the earth-stratum, which, while of variable thickness, and with no definite boundary, may yet be defined as that stratum of the air lying on the earth, which on the one hand is locally modified by the temperature and moisture of the soil, and on the other by the

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local agitation of the air due to retardation, and its contact with obstacles.

Of the law which expresses the acceleration of the wind as we ascend from the earth, almost nothing is known, and yet a knowledge of it, sufficiently exact to enable a calculation of the mean velocity of a horizontal current to be made, is essential to a comprehension of the forces which move great masses of air from place to place. Of the manner in which the change in direction from one wind to another takes place we are very ignorant, and shall remain so till sensitive instruments, sufficiently elevated to free them from the convolutions and gyrations due to retardation, are carefully observed and compared. The same may be said of the nature of the intermittent forces, often of great intensity, which give rise to gusty and squally weather. Such forces, or their manifestations, can only be examined with any hope of success, in positions far removed from local complications.

But perhaps the most important work which a meteorological tower like that I have described would render possible, is an examination of ascending and descending currents. The existence of such currents is undoubted, and the mightiest dynamic results are ascribed to them; but of their velocity and volume nothing is known inductively. As long as the observer is on the ground, the attempt to measure an ascending column of air is futile; but at a considerable elevation, if sufficiently isolated from disturbances of all kinds, there should be no insurmountable difficulty in contriving apparatus which would give both the direction and force of such a rising column. The gain to physical meteorology, which positive knowledge on this subject would bring, can hardly be over-estimated.

It may be urged that any elevation which a tower can give, is, after all, but a small fraction of the height of the atmosphere, and the observer is still virtually on the ground. In reply it should be said, that, compared with the present practice, such elevations are by no means insignificant, and, while greater heights are possible in balloons, the advantages they offer are very much more than compensated for by the systematic character of the work made feasible by the tower. It seems probable that when experience has fixed for each group of varying conditions the mean forms of the curve, as far as actual observation goes, it can then be carried to much greater heights by calculation. Errors may still exist,

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but in any case the approximation will be much nearer the truth than if the same were now attempted solely from readings made upon the earth. And, when such a system is fully developed, when physical observatories for meteorology exist in different parts of the country, as those for astronomy do now, may we not hope that the relations between the upper regions of the atmosphere and those below may be so well established that the necessary weather observations will acquire a new value, and enable deductions to be made which are unattainable at present.

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## On the Determination of the True Rainfall, by Elevated Gauges. By Francis E. Nipher, of St. Louis, Mo.

Somewhat over a century ago, the theory gained currency that a considerable portion of the rain which falls upon the surface of the earth was condensed from the lower strata of the atmosphere, within one or two hundred feet of the ground, and did not fall from the clouds as would be naturally supposed. This idea grew out of the observed fact that rain-gauges, placed at different elevations above the ground, showed a less rainfall as the gauges were placed at higher elevations.

Howard, Dr. Traill, and others, suggested that this apparent decrease in rainfall for higher elevations was caused by wind, and in 1861[1] Jevons proved that condensation in the lower strata is in all possible cases inappreciable, and pointed out with clearness the action of the wind in robbing the elevated gauges of their rain. His explanation is, briefly, that any obstacle placed in a stream of air causes the air to sweep around the sides and over the top of the obstacle with an increased velocity. As a result, the drops, which but for this disturbance in the air current would fall into the gauge, are deflected from their paths and no longer fall parallel to the general direction of the paths in the undisturbed stream. The rain drops evidently drift to the leeward. This explanation gives

[1] Phil. Mag., Dec., 1861.

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the key to the experimental results of Symons, who found that cylindrical gauges gave the best results, and that gauges of less diameter than two and three-fourths inches gave too small a result. The conclusion which Jevons draws, is that observations by rain gauges, elevated and exposed to the wind, must be rejected as worse than useless.

Nine years later (1870), the Rain Committee of the British Association made a report at the Liverpool Meeting. The results of the experiments of the committee wholly confirm the explanation of Jevons. It was found that when gauges were so arranged that no insplashing from the ground occurred, the observed rainfall (amount of water actually caught) increased as the gauge was brought nearer the surface, and reached a maximum when the mouth of the gauge was even with the ground. Insplashing was prevented by placing the gauge in a pit. The committee call the gauge thus placed in a pit, with its mouth on a level with the surface, a "pit-gauge." Their report in regard to the proper exposure of gauges, concludes as follows:—

"The inquiry we have just been pursuing naturally leads to the consideration of the best height for rain-gauges; and although a novel plan, we are inclined to consider that, theoretically, the 'pit-gauge' is the best. But there are several strong grounds of objection to it and to all gauges whose orifices are nearly level with the soil. We may mention a few:—

1. They become more or less filled with leaves during the autumn months, and their accuracy is thereby vitiated.

2. They become buried under snow, and the melting snow runs unfairly into them.

3. In cases of *extremely* heavy rain they collect water running along the surface of the ground, and sometimes become filled, at others float away on the water.

4. They are more liable to accidental injury and to collect all kinds of rubbish than if slightly elevated.

5. It is evident, from the tables, that an error of one inch in the height of a gauge near the surface is of as much importance as an error of one foot at a slight distance above it."[2]

In his paper "On the Deficiency of Rain in Elevated Rain-Gauges as caused by Wind" (*loc. cit.*), Jevons suggested another form of gauge which seems not to have received the attention it

[2] Which admits of evident explanation on the hypothesis of Jevons.

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deserves, although it must be admitted that the precise form which Jevons describes is open to objection. He says:—

"The most unexceptional rain-gauge would consist of a sheet of metal many feet square (for instance ten feet), spread flat upon the ground in an open place, with a flat collecting vessel in the centre connected by a pipe with a sunken reservoir, or recording apparatus. The edges of the vessel should not be higher than an inch, so as to present no appreciable obstacle or hollow space to the wind. At the same time nothing would be lost by splashing, as the splashes within and without the vessel would be equal."

The one objection which we have to urge against the accuracy of this gauge is, that during heavy rains, an accumulation of water in the collecting vessel might render the splashing within and without the gauge unequal.

It probably occurred simultaneously to my colleague, Prof. Woodward, and to myself, that this difficulty would be completely met by covering Prof. Jevons' metal sheet with square cells of equal size, each drained by equal openings at the bottom, one or more of those near the centre being used as collectors, the others being used only to secure uniformity of splashing. This gauge seems to me to be practically perfect, and although it can never be generally used, is, nevertheless, of the utmost importance for purposes of comparison with the more generally used, but certainly less accurate forms. Thus far we have been unable to find a proper exposure for such a gauge in St. Louis. It is hoped that this will be accomplished during the coming year, and that some person more favorably situated will also make a similar comparison.

If Jevons' idea be true it is evident:—

1. That gauges equally elevated above the ground are not necessarily directly comparable; they must also be equally exposed to the winds.

2. The problem of determining the true rainfall resolves itself into this: how shall a gauge be protected from the wind?

One of the most helpful rules in locating a gauge on the ground is to place it where light, newly fallen snow does not drift along the surface. A large court-yard entirely surrounded by buildings is a most excellent location. But there are many important stations where it is impossible to secure a ground exposure of any kind, and hence the desirability of securing this end by other means.

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After much consideration, it occurred to me that by shielding the gauge from the wind by some device which would deflect the wind downwards instead of allowing it to sweep up over the mouth of the gauge, the conditions of the pit-gauge of the B. A. Committee might be realized.

After using various forms, with more or less success, the shield now to be described was adopted as satisfactory.

The gauge is a cylinder, 7·08 c. m. in diameter,[3] and 18 inches long. Six inches from the lower end of the cylinder a false bottom is placed, and the cylinder is set over a turned post for support. Around this gauge a trumpet-shaped shield is placed, with the mouth or flaring part up. The shield is furnished with a clamp-screw at the bottom, and is braced near the top by metal strips set with their edges up and reaching from the shield to a sliding collar which encircles the gauge. By this means the shield can be set at any desired altitude on the gauge. The upper part of the shield terminates in a horizontal amulus of copper wire-cloth. Copper is the best metal for this purpose, as it is always wet by the water, and the meshes fill, which aids in deflecting the wind downwards. Furthermore, all splashing is avoided, as the drops fall through the cloth. In order that this end may be secured the wire must be small (No. 20, B. W. G.), the meshes running about eight to the inch. Water falling obliquely on this cloth is beautifully refracted on passing through.

Two gauges were set at an altitude of six feet above the surface, one being unprotected, the other being encircled by a shield, the top rim of which was on a level with the mouth of the gauge. The gauges were exposed in a yard surrounded on all sides by buildings, with the exception of a narrow opening to the north, and somewhat wider openings to the south and west. These openings were partly closed by lattice fences.

During the last summer and part of the spring, the unshielded gauge has shown a rain fall of ninety-seven per cent. of that shown by the shield-gauge. In the determinations of the B. A. Committee, it was found that an eight-inch gauge exposed at this elevation in an *open* place, gave about ninety-five per cent. of the rainfall of the "pit-gauge."

The results of these determinations are shown in the table,

[3] A cubic centimeter of water fills this gauge to a height of 0·01 inch. Of course the shield can be adapted to any form of gauge.

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where $s$ indicates the rainfall in the shielded ground gauge, and $u$ indicates the corresponding values of the unshielded gauge.

In order to test the efficiency of the shield more severely, sixteen gauges were mounted on the roof of the University tower, 100 feet above the ground. As had been anticipated, the results of the different gauges were very different, depending upon the direction and force of the wind. Without stopping to detail the

| $e$ | $p$ | $s$ | $u$ |
|---|---|---|---|
| 0·5 | 0·74 | 0·73 | 0·74 |
| 0·5 | 0·41 | 0·44 | 0·43 |
| 0·5 | 0·63 | 0·71 | 0·79 |
| 0·75 | 0·80 | 0·99 | 0·92 |
| 1·0 | 0·37 | 0·48 | 0·47 |
| 1·9 | 3·03 | 3·28 | 3·14 |
| 1·9 | 0·12 | 0·15 | 0·14 |
| 2·5 | 0·56 | 0·72 | 0·74 |
| 2·5 | 1·30 | 1·35 | 1·30 |
| 2·5 | 0·23 | 0·29 | 0·25 |
| 2·7 | 0·05 | 0·05 | 0·03 |
| 3·0 | 0·26 | 0·29 | 0·26 |
| 3·0 | 0·58 | 0·61 | 0·59 |
| 3·0 | 0·34 | 0·38 | 0·34 |
| 3·0 | 0·25 | 0·20 | 0·22 |
| 3·0 | 0·39 | 0·41 | 0·40 |
| 3·0 | 0·28 | 0 27 | 0·28 |
| 3·5 | 0·46 | 0·46 | 0·46 |
| 3·5 | 0·14 | 0·16 | 0·14 |
| 3·5 | 0·16 | 0·18 | 0·16 |
| 3·7 | 0·50 | 0·50 | 0·48 |
| 3·7 | 1·46 | 1·53 | 1·51 |
| 4·0 | 0·34 | 0·39 | 0·38 |
| 4·0 | 3·56 | 3·71 | 3·52 |
| 4·0 | 0 36 | 0·40 | 0·40 |
| 4·9 | 0·15 | 0·19 | 0·17 |

experiments made upon the roof, which confirm the results reached by others, the conclusions to which we were forced may be briefly stated:—

1. In all cases, with a single exception, the shields were advantageous.

2. It was impossible to find an exposure on the roof where a gauge gave uniform results. Those on the lee side frequently

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gave the same result as the ground gauge, but it seemed to be wholly a matter of accident.

Gauges placed centrally on a large, square and flat roof will doubtless give better results. Being unable to secure such an exposure on the roof of the University tower, the gauge was raised to an elevation of eighteen feet above the roof, and 118 feet above the ground. The unshielded gauge thus placed showed a loss of ten to fifty per cent. of the shielded ground gauge.

The shield was placed around the gauge and clamped with its rim at various elevations above the top of the gauge. When this elevation amounts to an inch, it is necessary to take precautions against splashing into the gauge. This was done by placing inside the shield a concentric cone of copper wire-cloth, separated from the shield by an interspace of half an inch. Equally effective is a wooden cone similarly placed, the inside surface being lined with bristles similar to those used in the manufacture of counter-brushes, and so arranged that the bristles stand vertically. Either of these methods is effective.

The results of the determinations are given in the table, where $e$ denotes the elevation of the shield above the top of the gauge, and $p$ the rainfall as shown by the shielded pole gauge.

It appears that when the shield is placed at an elevation of three and one-fourth inches, the pole gauge gives the same indications as the common gauge at the ground, although differing in level 112 feet. There is little doubt that raising the pole gauge higher above the roof will cause it to approximate more nearly to the true rainfall. The disturbing effect of the building is the only source of trouble.

It appears therefore: —

1. That the so-called correction due to altitudes of a rain-gauge, may be reduced to *nil*, by properly shielding the gauge from the wind.

2. That the gauge may then have any convenient altitude when not upon a roof. On the roof of a building it is probable that if the shielded gauge be properly elevated, the true ground rainfall will also be obtained. This point will receive further attention.

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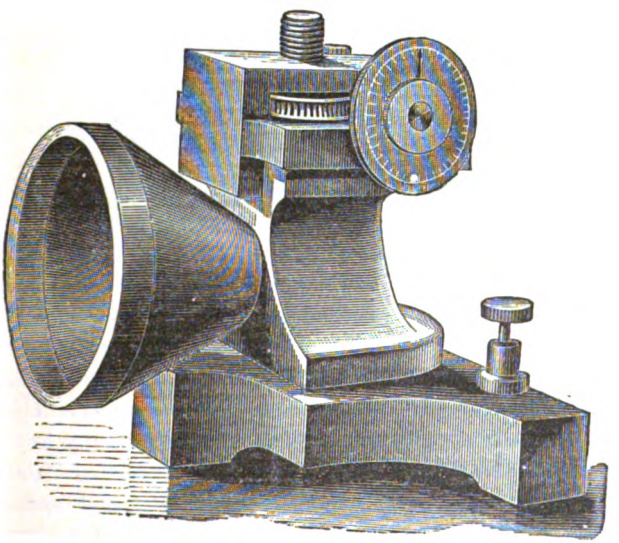
## On the Use of the Tasimeter for Measuring the Heat of the Stars and of the Sun's Corona. By Thomas A. Edison, of Menlo Park, N. J. (Being a Report to Dr. Henry Draper, and presented by his permission).

*To Prof. Henry Draper, M.D., Director of the Draper Eclipse Expedition:*

Dear Sir: The instrument which I used at Rawlins, Wyoming, during the solar eclipse of July 29, 1878, for the purpose of measuring the heat of the sun's corona, was devised by me a short time only before that event, and the time was insufficient to give it as thorough a test as was desirable to ascertain its full capabilities and characteristics.

This instrument I have named the tasimeter, from the Greek words τασις, extension and μετρον, measure, because primarily the effect is to measure extension of any kind. The form of instrument which I used is shown in the annexed wood cut (Fig. 1.)

Fig. 1.



With this instrument was used a Thomson's reflecting galvanometer on a tripod having a resistance of three-fourths of an ohm. The galvanometer was placed in the bridge wire of a Wheatstone balance, two of the branches of which had constant resistances of ten ohms each, while of the other two one had a constant of

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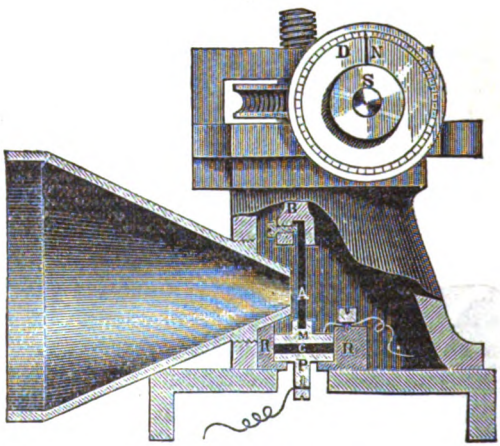
three ohms, and the other contained the tasimeter which was adjusted by means of the screw to three ohms. When thus balanced, if the strip of vulcanized rubber placed between the fixed point and the carbon button (seen in Fig. 2), was exposed to heat from any source, it expanded, placing pressure upon the carbon button thus decreasing its resistance and destroying the balance; thus allowing a current to pass through the bridge wire containing the galvanometer, the amount of this current of course being proportional to the expansion of the rubber and to the strength of the battery.

The form of instrument here described was only finished two days before leaving for the west; hence, I was unable to test it.

Fig. 2.




However, I set it up upon my arrival at Rawlins, but found that it was a very difficult matter to balance so delicate an instrument as a reflecting galvanometer with one cell of battery, through such small resistances. In fact, I did not succeed in balancing it at all in the usual way. Nor could it be balanced in any way until I devised a method which I may designate "fractional balancing," when it became very easy to accomplish the result and also to increase the effect by using two cells in place of a single one. This device consisted of a rheostat formed of two rows of pins. The rows were about one-half an inch apart. A wire was connected from a pin on one row to a pin on the other row and so on, so that the current had to pass through the whole length of the wire, which was No. 24 gauge and four feet long. This was used

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as a shunt around the galvanometer. A copper wire connecting all the pins of one row served to reduce the resistance to zero. When the galvanometer was thus shunted, a very feeble current passed through it. If the spot of light was not at zero it was brought there by either increasing or decreasing the pressure upon the vulcanite of the tasimeter by the adjusting nut. When thus brought to zero the copper wire of the shunt rheostat was taken off of one pin, thus increasing the resistance of the shunt perhaps to one-fiftieth of an ohm. The spot of light was generally deflected nearly off of the scale. The light was again brought to zero by varying the resistance of the tasimeter, and another one-half inch of wire included in the shunt, another deflection and another balance was obtained by the tasimeter. Thus by gradually increasing the delicacy of the galvanometer by increasing the resistance of the shunt and balancing at every increase, the whole of the current was allowed to pass through the galvanometer and the shunt taken off. When this point was reached the damping magnet or director was in close proximity to the case of the galvanometer. To increase its delicacy to the fullest extent it became necessary to raise the director to the top of the rod. This was done by raising it cautiously a quarter of an inch at a time, bringing the spot of light to zero each time by the tasimeter.

In order to form some idea of the delicacy of the apparatus when thus adjusted, a preliminary experiment was made on the evening of the 27th, with the star Arcturus. The tasimeter being attached to the telescope, the image of the star was brought on the vulcanized rubber. The spot of light from the galvanometer moved to the side of heat. After some minor adjustments, five uniform and successive deflections were obtained with the instrument, as the light of the star was allowed to fall on the vulcanite to produce the deflection, or was screened off to allow of a return to zero.

It was in this condition when the eclipse occurred. The tasimeter was placed in a double tin case, with water at the temperature of the air between each case. This case was secured to a Dollond telescope of four inches aperture. No eye-piece was used. At the moment of totality the spot of light was slowly passing towards cold. When I withdrew a tin screen and allowed the edge of the luminous corona to fall upon the rubber, the spot of light stopped, went gradually off of the scale towards heat, its velocity accelerating as it approached the end. The time required for the light to leave the scale was from four to five seconds.

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I interposed the screen and endeavored to bring the light back to zero, but I was unsuccessful. Had I known that the heat was so great I should have used a platinum strip in place of the vulcanite, and decreased the delicacy of the galvanometer by the approach of the damping magnet.

I then would doubtless have succeeded in getting two or more readings, and afterwards by comparison with bodies of known temperature would have obtained a near approach to the temperature of the sun's corona.

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THE SONOROUS VOLTAMETER. By THOMAS A. EDISON, of Menlo Park, New Jersey.

THE sonorous or bubble voltameter consists of an electrolytic cell with two electrodes, one in free contact with a standard decomposable solution and the other completely insulated by vulcanized rubber except two small apertures, one of which gives the solution free access to the insulated electrode, and the other allows the escape of bubbles of hydrogen as they are evolved by electrolysis. With a given current and a given resistance a bubble is obtained each second, which is seen at the moment of rise and which at the same time gives a sound when it reaches the air. The resistance may be increased so as to give one bubble in one, five, ten or fifty seconds, or in as many hours. I have compared this instrument with the ordinary voltameter, and find it much more accurate. By the use of a very small insulated electrode and but one aperture, through which both the gas and water current must pass, great increase of resistance takes place at the moment when the bubble is forming; and just before it rises, a sounder magnet included within the battery circuit opens, closing again when the bubble escapes, thus allowing by means of a Morse register the time of the bubble to be recorded automatically. This apparatus, when properly made, will be found very reliable and useful in some kinds of work, such as measuring the electro-motive force of batteries, etc. By shunting the voltameter and using a recorder it becomes a measurer not only of the current passing at the time, but also of that which has passed through a circuit from any source during a given interval.

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## On the Results of the Spectroscopic Observation of the Solar Eclipse of July 29, 1878.[1] By George F. Barker, of Philadelphia, Pa.

*To Prof. Henry Draper, M.D., Director of the Draper Eclipse Expedition:—*

Dear Sir: I beg leave to submit to you herewith my report on the spectroscopic observations made, and on the results obtained, at Rawlins, Wyoming Territory, during the solar eclipse of July 29, 1878, that portion of the work having been allotted to me by yourself in the organization of the expedition.

The instruments and apparatus used in the observations were loaned for the purpose from the physical cabinet of the University of Pennsylvania. They consisted (1) of an equatorially mounted achromatic telescope of four inches aperture made by Jones of London; (2) a direct vision astronomical spectroscope by Merz of Munich; (3) a second direct vision spectroscope by Hoffman of Paris; (4) and a pocket spectroscope by Geo. Wale & Co. Beside this spectroscopic outfit, a second four-inch achromatic telescope by Dollond was taken for use with the tasimeter by Dr. Edison, and a Savart, a Senarmont, and an Arago polariscope, for determining the polarization of the corona. The Merz spectroscope above mentioned is described in the "Philosophical Magazine," IV, xli, Feb., 1871. It is provided with two compound direct vision prisms, of which one or both can be used at pleasure, each consisting of five single prisms, two of flint glass with a refracting angle of 84°, and three of crown; one of these having a refracting angle of 84°, the others of 87°. The dispersive power of each of these compound prisms is about equal to that of two equilateral prisms of flint glass. The instrument has a collimating and an observing telescope, each furnished with an object glass two-thirds of an inch in aperture and four inches in focal length. The prism-tube is attached to the collimator by two centres, giving it a lateral motion about a line passing through these centres, which constitutes an axis parallel to the slit. The observing telescope is similarly attached to the tube carrying the prism. These motions serve to alter the incidence of the rays upon the surface of the prism, and also to bring any special part of the spectrum into the middle of the field. The observing tele-

[1] Presented by permission of Dr. Draper.



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scope is provided with a positive eye-piece of an equivalent focal length of one inch, and also with a needle micrometer, having an eye-piece of one-half inch focus. The graduations upon this micrometer are strongly cut, enabling the positions and the distances of the lines measured with it to be easily read even in a faint light. The spectroscope was firmly attached to the draw tube of the equatorial telescope by means of an open frame made by Zentmayer, so that the position of the image with reference to the slit could be readily observed.

The time from the 19th of July, the date of our arrival at Rawlins, until the 29th, was occupied in setting up the instruments, in getting them into adjustment, and in practice with them. It was found that with only one of the compound prisms of the Merz spectroscope, the slit being placed radially, it was easy to observe the lines C and F reversed in the chromosphere, and also the bright line $D_3$. On the morning of the day of the eclipse, the solar edge was examined for protuberances, in order to locate them in advance of totality. But a single one was noticed, this being on the southwestern edge of the sun. As the time of first contact approached, the spectroscope was removed and a paper screen was attached to the draw-tube, an image of the sun being formed on this screen by means of the eye-piece; thus enabling the time of this contact to be approximately determined and the subsequent progress of the eclipse to be conveniently observed. No spots were seen under these circumstances, though this could hardly have been expected since the solar image was so small, scarcely three inches in diameter, unless the spots were of large size. As the time of second contact drew near, the spectroscope was replaced upon the equatorial. Since you deemed it of importance to pay special attention to the oxygen lines in the vicinity of G, the micrometer of this instrument was, at your suggestion, so adjusted that one of its needle-points rested on the hydrogen line near G and the other on the line known as *h*. After the last ray of sunlight had disappeared, I took a few seconds of the precious time to observe the eclipse with the naked eye. The moon appeared intensely black, surrounded by a pinkish halo, extending to about two-fifths of a lunar diameter from the limb, and occupying the entire circumference. At two points this halo was expanded into radial streamers, one of which had parallel sides with a deeply indented or swallow-tailed end, ex-

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tending westward of the sun and apparently lying in the ecliptic; the other appeared single, was on the eastern edge, and was inclined twenty degrees or more to the north of the ecliptic. The former of these streamers was traced to a distance of about a lunar diameter from the edge, the latter to a somewhat less distance. No structure could be seen in the halo, but in the streamers traces of parallel rays appeared to be present. The amount of light emitted by the corona was a surprise to me. Preparations had been made for using artificial light for reading the circles, but this was found not to be at all necessary. The amount of light seemed to be nearly or quite equal to that given by the moon when ten days old. No protuberances were seen with the naked eye; nor were any streamers observed, other than those already described. A glance at the eclipsed sun was then taken through the finder of the equatorial. The magnifying power being low, the corona presented much the same appearance as to the naked eye; but the streamers showed much more distinct evidences of a radiated structure and a pale rosy protuberance was observed on the southwestern edge of the dark disk. This was undoubtedly the same prominence which was observed previous to totality.

Turning my attention now to the spectroscope, upon the slit of which the coronal image had already been brought by means of the finder, the slit being placed radially, the first glance through the instrument showed me a bright, but an absolutely continuous spectrum. The region under examination was of course that portion of the spectrum which had been placed before totality between the needle-points of the micrometer. Totally unprepared for so unexpected a result, I moved the observing telescope so as to bring the green portion of the spectrum into the field, expecting certainly to see 1474 K, and by the appearance of this line to determine whether my instrument was out of adjustment; and if it were, to adjust it again. But no bright line was there; the green region appeared as continuous as the blue. I then gradually closed the slit—which had been previously adjusted on the solar spectrum so that the line D appeared nebulous on its edges—thinking that I might in this way improve the definition, but with no better results; no bright lines could be seen. To my great surprise, however, when the slit was thus narrowed, the region which was then under examination, that extending from *b* to G, appeared filled with dark lines on the brighter background

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these dark lines being readily recognized from their general appearance as the solar lines of Fraunhofer. Still intent on getting bright lines, I opened the slit again gradually, moved the observing telescope over the entire length of the spectrum from red to violet, repeating the operation three times and varying the width of the slit from time to time in each region; but not a single bright line could be detected. I then requested you to come and take a glance through my spectroscope, as had been previously agreed; saying that although I could see dark lines and a continuous spectrum, I was unable to detect a single bright line, and knew not what to make of it. You were then looking at the eclipse through your ingenious little telespectroscope of two inches aperture. You came to my instrument, looked at the spectrum, moved the observing telescope over its whole length and remarked that the results in my spectroscope agreed entirely with those in yours, and that the spectrum appeared in both absolutely continuous.

My mind being thus relieved, I took my place again at the spectroscope, and this time, placing the slit tangential to the moon's limb, I moved the observing telescope from end to end of the spectrum, opening and closing the slit at intervals; but the spectrum appeared as continuous as before. Again the image was adjusted so that the slit was once more radial; and this time on still a different portion of the corona. On examining again the spectrum, no bright lines appeared, except once for an instant, when the slit passed over the small chromospheric prominence already noticed. Warned by Mrs. Draper's clear and distinct counting that the precious 165 seconds had two-thirds gone, I decided to devote the time still remaining to a more careful observation of the dark Fraunhofer lines. Now, for the first time, as I adjusted the width of the slit and its position on the corona with more care, I observed that these lines did not pass clear across the field, but were of a length corresponding to the width of the coronal image on the slit. At the base of the spectrum, which corresponded to the base of the corona, they appeared bright and sharp; certainly quite as much so as in the light of the moon similarly condensed; though the continuous spectrum which formed their background was relatively brighter than in moonlight. There was no difficulty in identifying them as Fraunhofer lines from their general appearance and position; but some of them could be iden-

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tified beyond question. Such were *b* and F, which were especially distinct, D, E, and G, which were considerably less so. They faded gradually out from the base of the spectrum upward, appearing to end where the continuous spectrum of the corona was limited above. While thus employed, a flash of sunlight told us that totality had ended and that the solar eclipse of 1878 was over.

In discussing the results of the spectroscopic observations which have now been detailed, I am, in the first place, quite at a loss to account for the fact that no bright lines were seen by me, notwithstanding the persistent efforts made to get them. The failure to observe them can be accounted for, as it would seem, only on the ground that with the dispersive power employed, the bright lines were too faint to be seen on the much brighter background of the continuous spectrum.

The lessons to be drawn from these spectroscopic observations appear to be few and simple. The absence of bright lines, or at least of any which were at all brilliant, proves clearly the absence in the solar coronal region of any considerable mass of incandescent gas or vapor; which shining by its own light would of course give a bright line spectrum. The presence of Fraunhofer lines in the coronal spectrum shows conclusively the presence of reflected sunlight in the light of the corona and goes to establish the theory long ago suggested, that masses of meteoric matter raining down upon the solar surface from all directions, reflected to us the light of the sun and were therefore the essential cause of the coronal phenomena. And, finally, the fact of the increased brightness of the continuous spectrum, as compared with the intensity of the dark lines of Fraunhofer, goes to strengthen the probability that there is still other light in the corona which comes to us from the incandescent liquid or solid matter of these incandescent meteoric masses. These conclusions, deduced very simply from my own spectroscopic results, agree completely, I am happy to find, with those drawn from your most excellent photographs, as well as from the ingenious heat-measurements of Dr. Edison and the polariscopic determinations of Dr. Morton.

In concluding this report you will permit me, I am sure, to express the great gratification which the Draper Eclipse Expedition has continually afforded me. The delight at being able to witness a total eclipse of the sun has been intensified by the complete success of all the attempts made to observe it, and by the most agree-

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able companionship during the trip. My obligation to you for the opportunity is as profound as my thanks to you for it are sincere and cordial.

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## On a New Method of Measuring the Pitch of a Tuning Fork. By George F. Barker, of Philadelphia, Pa.

The methods which have thus far been employed to estimate directly the absolute pitch of a tuning fork are for the most part graphic methods, in which the fork records its own rate of vibration. Of these, the phonautographic method as improved by Kœnig, and the modified form of it devised by Müller, are probably the best. In the former, a light compound style, consisting of a bristle tipped with a piece of feather, is attached by means of wax to one prong of the fork to be measured. In the latter, a style of thin metal is similarly attached. Since a similar weight must be attached to the second prong to balance the first, and since a fork loaded in this way must have a different rate from that which it has when vibrating free, I have been led to propose a method for recording automatically the pitch of a tuning fork, without interfering in any way with its normal rate. The method rests on the principle of the induction of a magneto-electric current by the motion of a vibrating magnetic body. The fork to be measured is first magnetized and is then vibrated by means of the bow, in close proximity to the pole of an electro-magnet of suitable resistance. Electric currents are thus generated, alternate in direction, which flow through the circuit. It remains to devise a method for recording these alternations of current, the rapidity of which represents the pitch of the fork. Since the currents generated are feeble, the means of recording them must be correspondingly sensitive. For currents of considerable strength the record may be readily obtained by the ordinary chemical telegraph method, in the form known as the automatic. A metal pulley, revolved by a multiplying wheel, furnished with guides by which a strip of paper moistened with a solution of potassium ferricyanide may be kept upon it and carried along with it as it revolves, is connected with

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the negative electrode, a piece of iron wire being connected with the positive. Whenever the current passes, the iron being in contact with the paper, a blue stain appears, continuous if the current is so, interrupted if the current is intermittent. In the fork experiment a sufficiently strong current would show a series of dots on the paper, each one corresponding to one complete vibration of the fork.

This method is not sufficiently delicate, however, for the matter in question. For an improvement upon it I am indebted to a suggestion of my friend Dr. Edison, based upon a discovery of his that tellurium when used in this way is excessively sensitive to the electrolytic action of hydrogen. A minute point of tellurium, connected with the negative electrode, and drawn over moistened paper, leaves a trace, which, if the motion be exceedingly rapid, is reddish-brown in color close to the point, but which becomes almost instantly a jet-black, and is permanent. No effect at all results if the positive electrode be connected with the tellurium. Moreover, this action is not only exceedingly delicate, so that it may be used with very feeble currents, but it is also exceedingly prompt, as many as 1200 distinct dots having been made with it in the space of a single inch; thus enabling it to serve admirably for the research under discussion.

The apparatus required for the method of measurement now proposed, will consist as follows: first, of a suitable electro-magnet, whose resistance has been determined. Second, of the fork to be measured, sufficiently magnetized, and clamped so that one of its prongs may vibrate close to one of the poles of the electro-magnet. Third, of a roller, driven by a multiplying wheel or by clock-work, carrying on its face a strip of moistened paper, and connected by a wire to the electro-magnet. Fourth, of an iron wire, tipped with tellurium, held so that its point may press on the paper, and so connected to the electro-magnet that the point represents the negative electrode. And fifth, of an apparatus for comparison, which may be either a seconds pendulum, in cases where the vibration numbers are low, or an electro-magnetically vibrated fork, intermediate in rapidity, whose rate has been carefully determined in comparison with the pendulum, by the present method, which shall make the absolute time-record on the same strips of paper by means of a similar recording apparatus. To use the apparatus, one wire of the electro-magnet is connected with the tellurium pen,

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the other with the roller, over which the moistened paper passes, the second tellurium pen being placed in the circuit of the battery working the comparison fork, and suitably shunted. The fork is magnetized and so adjusted as to vibrate in close proximity to the electro-magnet, when it is bowed. Of course it is immaterial what the polarity of the prong nearest the magnet is, since its vibration toward this magnet generates currents opposite in direction from those produced when it vibrates from it, and one or the other of these will be negative and produce the stain with the tellurium pen, thus registering only complete vibrations. The comparison fork is now set into vibration, and the adjustments necessary to get a sharp and clear record on the paper, are made. The fork to be measured is then bowed, and the like adjustments made. Both forks are then made to vibrate, and different lengths of the paper are run off in the same interval; say one, five, ten, fifteen, or twenty feet in a second. Subsequent comparison will give the ratio of the vibration numbers in the two cases; and knowing the absolute number given by the comparison fork, those of the other are easily calculated.

It may be objected to the method of measurement now described that the results obtained by it may be somewhat inaccurate, owing to the rate of the fork being changed by its magnetization. Although direct experiment has shown that this objection has no real weight, and that the rate of the fork is sensibly the same both before and after magnetization, yet it seemed worth while to alter the form of the experiment to avoid this possible cause of error. This is easily and very simply done. In place of using a magnetized fork, the fork is used in its ordinary unmagnetized condition. A more or less feeble battery current is caused to flow through the electro-magnet, the direction of this current being fixed by the fact that the pen, whether shunted or not, must be connected with the negative electrode. When now the fork is caused to vibrate, the alternate approach and recession of one of its prongs to the magnet, will, by inducing a counter current, weaken or strengthen the primary current, and thus produce the dots upon the paper, the pen being suitably adjusted. Still a third arrangement may be preferred in special cases. In place of using a battery current, the electro-magnet may have a permanent steel magnet as its core. The vibration of an unmagnetized fork, close to the pole of such a magnetized core, would generate electric currents precisely in the

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same way as if the fork and not the core were magnetized. The same general form of apparatus would be used in each of these three methods.

It will be seen that the form of apparatus last described is essentially similar in principle to that involved in the construction of the magneto-electric telephone. Indeed, the latter instrument in any of the identical forms now so universally used, may be employed to measure the rate of vibration in cases where the vibrating body is not magnetic, *i. e.*, in the case of air, of a string, of a plate, etc. By sounding an organ pipe near a suitably constructed telephone of this sort, the pitch may be automatically recorded upon paper. Moreover, not only may the whole range of simple sounds be thus automatically recorded, and their pitch determined, but the harmonics of compound sounds may also be obtained. Indeed, as is well known, it was the paper record of spoken words, obtained by Dr. Edison by speaking into his carbon telephone, that led him to the construction of that marvellous instrument, the phonograph. Whether the paper record, made by tellurium, will ever rival in delicacy that of the phonograph, is a matter yet to be investigated.

Thus far, this method of measuring the pitch of a fork has been used only in a qualitative way for purposes of demonstration. A series of experiments now in progress, will establish its quantitative accuracy.

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## On a Modified Form of Telephone, intended to be used as a Sensitive Electroscope for the Detection of Feeble Currents. By P. H. Van der Weyde, of New York.

[ABSTRACT.]

The purpose aimed at being to increase the sensitiveness of the instrument so that the weakest current would affect it, the coil was wound in such a form as to act on the diaphragm directly, as well as with the intervention of the magnet. For this purpose the coil was wound like a flat disk, of nearly the same diameter as the diaphragm, and placed between it and a flat star-shaped

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magnet, with one north pole in the centre, and eight south poles more or less at the circumference. As the diaphragm is nearly in contact with the poles, all bent up to meet it, it is magnetic by induction, having south polarity in the centre and north polarity at its circumference, so that the coil is entirely surrounded by magnetic iron and steel, but every turn of the coil also very close to the magnetic metal. Experiments have proved the great delicacy of this arrangement, so that, for instance, currents in gas pipes were detected where other means failed.

This modification of the telephone is the invention of N. J. Van der Weyde, a civil engineer and son of the writer.

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## TITLES OF OTHER PAPERS READ IN SECTION A.

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A Windvane thoroughly Sensitive and thoroughly Steady. By William Curtis Taylor, of Philadelphia, Pa.

Magnetic Determinations in Missouri. By Francis E. Nipher, of St. Louis, Mo.

On Improvements in Electric Clock Systems. By Francis E. Nipher, of St. Louis, Mo.

On Finding the Differential of a Variable Quantity without the use of Infinitisimals or "Limits." By Joseph Ficklin, of Columbia, Mo.

On the Economic Use of Steam. By Charles A. Smith, of St. Louis, Mo.

A Peculiar case of Fracture of Tool Steel. By William Kent, of Pittsburgh, Pa.

On the Possibility of observing the Solar Corona without the aid of an eclipse. By Ormond Stone, of Cincinnati, Ohio.

On the applications of the Carbon Button. By Thomas A. Edison, of Menlo Park, N. J.

On the Principle involved in the Microphone and the Carbon Telephone. By Thomas A. Edison, of Menlo Park, N. J.

International Coinage. By E. B. Elliott, of Washington, D. C.

Internal Revenue Adjustment of the Carlisle Tables. By E. B. Elliott, of Washington, D. C.



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PERMANENT

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# ADDRESS

OF

# PROFESSOR FRANK WIGGLESWORTH CLARKE,

CHAIRMAN OF THE SUBSECTION OF CHEMISTRY.

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FELLOW MEMBERS OF THE SUBSECTION OF CHEMISTRY:—

ONCE a year it is our pleasure and our privilege to meet together, for the interchange of views upon the questions of chemical science; for the comparison of notes in our various lines of research; for mutual help, sympathy and improvement. It is also, I suppose, a part of our work to attract public attention to the subjects that interest us, and to do what we can to secure for chemistry a wider appreciation and greater means for development. No branch of science has done more for civilization than ours. Old industries have been revolutionized, and new ones created; things which were once the luxuries of the few have been made the daily necessities of the many; all arts and all manufactures owe tribute to the chemist. A comparatively small number of men, a majority of them teachers, working only in their intervals of leisure, established the principles which have brought about these wonderful results. If small means, widely scattered and unsystematically used, have wrought such marvels, what may we not expect from the greater opportunities which a more general comprehension of the value of our labors must eventually bring?

To the members of this subsection, the economical achievements of chemistry are familiar as household words. As we look about



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us in our daily lives, we see in every direction the fruits of chemical investigations. Every scrap of metal; all paints, varnishes, fats, oils and fertilizers; every bit of glass or porcelain; every cake of soap or box of matches, embodies some improvement which chemistry has made. Our linen is bleached and our outer garments are dyed with the products of the laboratory. Whether we burn candles, gas, or kerosene, we still have chemistry to thank for nearly all there is of cleanliness, convenience, brilliancy, purity, and cheapness in the light. In many articles of food, and in a long list of medicines; in the photograph and the galvanic battery; by the conversion of waste rubbish into articles of beauty and usefulness; in short, through a vast network of improvements and discoveries, our still infant science has established its claims to recognition as a benefactor of mankind. Would that the multitudes who have enjoyed these benefits might see their sources as clearly as we do! Then would science be fostered and encouraged, where now it struggles feebly to secure a grudging and scanty support.

A single discovery of the chemist may work peaceful revolutions in many departments of labor. Pardon me if I pause to illustrate this truism by a very familiar example. Even for us it is not a waste of time to look backward occasionally, and to consider the consequences developed from one great research or invention. Such considerations may well strengthen us in our hopes for the future of chemistry.

Nearly ninety years ago, Le Blanc discovered his famous process for converting common salt into soda. No process could be much simpler than this, and yet in its ramifications it has affected every branch of civilized society. In the first place, it widened the field of labor. Certain materials were needed at the start, namely: salt itself, charcoal, limestone, and sulphuric acid; and many workmen found employment in their production. By the new demand for sulphuric acid, this important compound was rendered cheaper, and every other chemical industry was thus directly facilitated. If the familiar saying be true, that the degree of civilization to which any country has attained may be measured by the amount of sulphuric acid it consumes, then the importance of this single item can scarcely be over-estimated. Passing from the materials employed to the process itself, we find that incidentally, as a by-product, it furnishes immense quantities of

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hydrochloric acid at a minimum of cost, and that here again all branches of manufacturing chemistry receive direct benefit. As a final result—if indeed, any result can properly be called final—we find that the soda, for which the process was devised, has been enormously cheapened. In 1814, soda crystals were worth about three hundred dollars per ton. By 1861, the price had fallen to twenty-two dollars; five thousand tons a week were produced, and ten thousand laborers found direct employment. This does not include the labor engaged in furnishing materials for the process, nor that incidentally stimulated through other industries. By the cheapening of soda, other things of more generally familiar utility were cheapened also. Chief among these we may mention glass and soap, two articles in which soda is a leading ingredient. Such a reduction in the price of soda as that just indicated could not but work wonders here. As glass and soap became cheaper, the demand for them naturally increased; and hence, through Le Blanc's invention, our houses are better lighted, cleanliness has been encouraged, and the public health, because of these steps forward, has unquestionably been improved. In short, the results of this single invention, direct and indirect, can be traced into nearly every department of human industry. These results have been exclusively beneficial. All of us share in their advantages, and no one has been injured. They have given employment to many thousands of laborers, and that without prejudice to older occupations. And thus it is in some degree with every discovery of the chemist. Each one is like a grain of corn, small in itself, and yet a germ from which may spring the food of numberless future generations. In the course of our labors, many grains may fall by the wayside and bring but small return; still, that which sinks into fertile soil will yield a thousand fold reward to the sower.

I need build no argument upon the facts I have just given. They stand before us, not only on the pages of books, but embodied in countless manufactories scattered all over the civilized world. They render life pleasanter, easier, more comfortable. They are the sources from which future discoveries shall flow, and help to make certain the steady growth of civilization. If the general public is not interested in chemistry, it is because we as chemists have neglected a part of our duty. We have but to speak, in order to command the public ear. Our work is work of



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national importance, and is sure in time of national recognition. Let us ask ourselves to-day how the splendid achievements of the past may be made more fruitful; and what measures and what researches will best advance the interests of our science in the future.

It is safe to assert that in every science there is some central line of growth, to which all details are subordinated, and along which its fundamental principles find their readiest and most logical development. Here lie the germs of future generalizations; whatever promise there may be of greater exactness, either in methods or in data; and all those deeper conceptions which most intimately connect a given science with other branches of thought and knowledge. Without such a main stem, each science would be but a mass of scattered details; isolated facts; fragmentary principles; with little coherence or order. True, we may fail to recognize a real line of growth, or we may follow a false clew, and yet make discoveries of great value; but without clear ideas upon this subject the highest work is impossible. In physics, the doctrines of the conservation of energy and the correlation of forces are points on the central line. In the study of organized life, the theory of evolution indicates a main stem. How is it with chemistry?

Among chemists to-day there seem to be two schools; at least practically, if not in point of abstract theory. It is almost as if the line of growth had divided, so that we can hardly tell which is the greater, and which the lesser stem. One school, represented by a large majority of modern working chemists, seems to take an interest only in the statical side of the science; its chief aim is to discover immense numbers of new compounds, and to theorize upon their constitution. Strangely enough, these chemists have devoted nine-tenths of their energy to the compounds of a single element, carbon; scarcely regarding other substances save in so far as they unite with this or with bodies containing it. To me it sometimes seems as if, in the light of their labors, chemistry was to be defined as the science of speculating upon the possible position of theoretical atoms within imaginary molecules. Do not think, however, that I underrate the value of the work they have done. I fully recognize its importance, although I consider it unfortunate that so much time and energy should have been spent in this one line of research, to the neglect of other, and I believe greater, fields of investigation.

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The other school of chemists, a school which seems to be rapidly growing in England and to be gaining industrious votaries elsewhere, may be described as essentially dynamical in its ideas. It sees in every chemical reaction three objects of study: first, the substances which enter into the reaction; secondly, the phenomena which occur during the reaction; thirdly, the substances produced by the reaction. The second term, the term which involves all the transformations of energy, is to them of at least equal importance with the others. In this they study the play of forces attendant upon the formation and destruction of compounds, the appearance or disappearance of heat, the velocity of the change, the effects of pressure, the alterations in volume, and so on. To them, every element and every reaction is important and interesting; they strive to see each change of composition in all its relations; to study processes as well as results; to recognize the intimate connection between chemistry and other sciences. Does it not seem as if these workers were pretty nearly on the right track? Is not their method of study the deepest and broadest? Does it not really include all there is of permanent value in the other school? To me at least it seems as if here the great advances are to be made, and the central line of growth discovered.

This feeling is justified, I believe, by history. Much of the best progress in chemical science has been made on the physical side. In truth, chemistry and physics are but one at bottom, having their roots in the same fundamental principles. Electricity cannot be studied apart from chemical considerations, neither can light nor heat. Nor are we able to understand truly a chemical operation until we know a good deal about the physical forces which it necessarily involves. Neither science can be mastered apart from the other; for they are but two great branches from one common line of growth. The molecular theory, without which modern chemistry could hardly have existed, rests mainly upon physical foundations. Apart from the laws of Avogadro and Ampere, of Dulong and Petit, of Boyle and Marriotte, of Gay Lussac and Charles, much which is now plain and orderly to the chemist would be chaotic and unmanageable. Doubtless, at some future time, the investigations of Mitscherlich and others into isomorphism, of Kopp into molecular volumes, of Thomsen and Berthelot into thermochemistry, of Gladstone into refractive indices, of Harcourt and Esson, and Boguski and Kajander into the

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velocity of chemical changes, will lead to generalizations of equal importance with these. The workers I have named, with many others equally worthy of naming, have already done enough to give us glimpses of great laws which we are as yet unable to see in their entirety. These glimpses forward are like promises made by Nature; sometime to be fulfilled, certainly never to be broken. Were we to take from chemistry all that has been done upon the physical side, how little true science would remain! We should have left but little law and little order; scarcely more than a vast mass of scattered facts and details, unconnected with any fundamental conceptions or any definite method of scientific research. Physics, stripped of its chemical portion, might make a somewhat better showing; but even then it would be little more than a skeleton of what it now is. Neither science can be strong without the other.

One of the main objects of science is to render prevision possible. The more thoroughly our knowledge is coördinated, the better are we able to predict the nearer discoveries of the future, and to see what lines of research will be most fruitful. Science is continually striving after exactness; of which this prevision is one of the results. The line, therefore, which leads most directly to definiteness and precision in any department of knowledge, is evidently the true line of growth which we should seek to find and to follow. Nature marks out pathways for us, even though we close our eyes to her indications.

To-day, notwithstanding its brilliant achievements in the past, chemistry is an inexact science. From top to bottom, from beginning to end, it shows signs of imperfection. Indeed, to most minds, this incompleteness is a great charm. There is so much to be done in chemistry, so many lines of research to follow out, so many discoveries to be made, so much glory to be won and good to be accomplished, that an active mind can hardly fail of being attracted. We may well feel, with the German philosopher, that the power to search after truth is more precious than truth itself. Were it possible for any science to become absolutely perfect, we should but idly fold our hands and enjoy its fruits, caring little for the history behind us, or for a future which could bring us nothing more. Perhaps, in the beneficence of Nature, we might even lapse into forgetfulness, in order that our descendants could enjoy the pleasure of a rediscovery. Speaking simply in a relative sense,

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chemistry is an eminently imperfect science. In experimental resources it is wonderfully rich; in delicate methods of investigation no other science can surpass it; but in those principles which render foresight possible, chemistry is poor and meagre. We may guess the existence of some undiscovered compound, but until we have prepared it what can we tell of its properties? A little perhaps, a very little; and that only approximately. Of its relations to the great forces of Nature, we know in advance almost nothing. We are able only to devise for it some "structural formula" which shall last for about five years, and then be replaced by another of equally perishable quality. We are, in fact, to-day but laying the foundations of a future science, which shall be to the chemistry of the present what that is to the alchemy of the past. What the chemist has already done for humanity has been done in spite of difficulties and defects; it is but a trifle compared with that which shall be accomplished in the better time to come. Enough has been done to prove the possibility of more; to encourage the investigator in his labors; to show the public that here is something worthy of study, of help, and of applause.

There are three great objects of investigation in chemistry as seen under its present aspects; three central problems upon which all else depends. First, what laws govern the transformations of energy that occur during chemical changes? Second, how do the properties of compounds stand related to those of the elements contained in them? Third, what is the nature of chemical union? These problems must be studied largely together; each one in the light of the other two. The first and second, however, are more practical in character than the third, and involve less speculation. They need for their solution severe experimental researches, the exact determination of numerical data, and reasoning of a rigidly mathematical kind. Under the third problem, we encounter the chief speculations of chemistry, hypotheses which serve to suggest and stimulate investigations with regard to the other two, while at the same time dependent upon these latter for the security of their foundations. The nature of the elements, whether one or many; the truth or falsity of the atomic theory; all questions of intermolecular structure; the validity of certain formulæ; the value of such doctrines as that of quantivalence; these are the ideas with which this problem has to do. Each one of these theoretical questions is but a special case depending for its answer upon our views con-

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cerning the main point of all. Plainly, the first thing for chemistry to do is to make sure of its foundations; using speculation only as a line by which parts of the work may possibly be helped and guided.

The only foundations which can stand the test of time, in such a science as ours, are exact, rigorous, quantitative measurements. We already know tolerably well in a qualitative way what transformations of energy occur in chemical changes; the question now is as to their definite numerical relations. How much heat appears or vanishes in any given reaction? what is the exact electromotive force of any specified couple? what laws of quantity connect actinic energy with particular combinations or decompositions? These are the questions that science now asks, and for which in a few cases we are just beginning to find answers. The physicist approaches these questions from one side, the chemist from another; eventually the two will meet, and a general solution will be found. From our knowledge of forces on the one hand, and of substances on the other, we shall become able to compute the dynamical relations of every possible chemical change, and perhaps even to determine in advance what reactions can and what cannot take place.

Although the foregoing statements relate specifically to the first of the three great problems, similar principles apply to the second. If we are to determine what general laws connect the properties of compounds with those of the elements contained in them, we must first secure a large mass of well-established data to work from. For a very large number of subtances, such constants as density, crystalline form, melting and boiling point, thermal conductivity, specific and latent heat, co-efficient of expansion, index of refraction, electrical resistance, and so on, ought to be rigorously ascertained. With such accurate data, we may hope to reach the desired laws; without them we can see but a little way. Moreover, every constant should be determined in a much more thorough manner than any have been determined hitherto. Every measurement should be repeated many times, and under all attainable conditions; otherwise the work would be manifestly incomplete.

Fragmentary researches of this kind have already been carried out in great number; but how? In some directions I have tried to collect the evidence, in order that we might know what data were really available, and where the worst gaps lay. The dis-

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crepancies which sometimes come to light are amazing. Take a large number of specific gravity determinations for any given compound, and see how discordant they are. The values will range through wide limits, so that it is often impossible to say which one out of several is the best. These variations may be due partly to bad work, and partly to unrecognized differences in chemical constitution. Some of them are so peculiar that they can hardly be explained, except upon the hypothesis that they spring from some undiscovered allotropy or isomerism. The metallic iodides furnish some good illustrations of this idea. The well-known properties of mercuric iodide, Rodwell's experiments upon silver iodide, and Cooke's researches into antimony iodide, are sufficiently suggestive. When, therefore, we meet with discordant specific gravity determinations among other iodides, we may reasonably suppose that one experimenter has dealt with one allotrope, and another with another; both unconscious of any existing differences. But however this may be, the discordance of so many data, not only among specific gravity measurements, but in all other series of determinations, serves to show how little we really know, how much work remains to be done freshly, and how much ought to be done over again. Of all the figures upon which we now rely for the solution of these grander problems, at least three-fourths should be scrupulously redetermined; and even after that had been done, our material would still be wofully insufficient. The kind of work we need may be well illustrated by the classical labors of Stas upon the atomic weights, and the immortal researches of Regnault into the physical properties of vapors. Such work as this will give imperishable foundations for exact science, strong enough and broad enough to sustain the most stately edifice which the human intellect can rear.

To my mind, the investigation of this sort most immediately needed is the accurate determination of all the physical constants for all the so-called elements. This would supply evidence directly related to all three of the great problems. We might thus learn something about the nature of the elements themselves, and how far the transformations of energy due to chemical action depended upon their individual characters; we should also be put in some position to predict the properties of compounds. In many important lines of research, science would then replace speculation; and the imagination could still find ample fields in

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which to exert its powers. As yet, such work as this has not been done for even one of the sixty-five or sixty-six elements known. Take iron, for example; the most important of the metals; one which has been the subject of numberless practical researches; and we as yet know it in these more purely scientific relations barely on the surface. Not a single series of physical constants has yet been thoroughly determined with regard to it, notwithstanding the light that such investigations might shed upon important industrial problems. We know a little here and a little there; a bit about its thermal properties and another bit about its electrical properties, but nothing systematically and well. As for gold, silver, copper and mercury, rather more is known; but even with these elements not a tenth of what ought to be done has really been accomplished.

The reason for such incompleteness in chemistry is sufficiently evident. Indeed, I have already hinted at it. The labor of investigation hitherto has fallen chiefly upon the shoulders of volunteers, who have worked in great measure independently of each other, every man following his own particular bent. Part of the work has been done by teachers, in their odd moments of freedom from the drudgery of instruction; other portions were carried out by inventors, or by manufacturers with reference to special industrial questions. There has been little coöperation, and no organization of efforts. The one class of investigators has sought for the glory which brilliant discoveries bring; the other has labored to secure pecuniary rewards. Thus has our pure science grown unsystematically, and our applied science has been imperfectly applied. The two depend upon each other; and the more complete pure science can be made, the simpler and more beneficent will its applications become.

What chemistry needs, then, is combined effort upon some general plan. There must be a body of trained workers under an efficient head, who shall mark out some of the lines for investigation to follow. Such a coöperation can hardly be brought about under existing circumstances and with present facilities, since as a matter of course every scientific man prefers to work independently, and to shape his researches for himself. Something may be done, perhaps, by those college professors who are fortunate enough to have a considerable number of advanced students to assist them; but work carried forward under such conditions can

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scarcely amount to more than a clearing of the ground. Still, such pioneer labor is not to be despised. It marks out many useful pathways for us, and shows where the chief difficulties are to be encountered. The one great method, however, by which the desired ends may be attained, is to be found in the establishment of endowed laboratories devoted to pure research. With regard to laboratories of this kind much has been written, both for and against. From two distinct points of view has the subject been treated. One set of writers has opposed the idea of endowing research, upon what appear to me to be wholly fallacious grounds. They have urged that the payment of salaries to scientific men, either as a reward for past services, or as a means of encouraging future work, would probably result in the foundation of sinecures in which pretentious idlers might find an excuse for doing nothing. A man could easily pretend to make investigations, and in reality only fritter away his time in a sort of elegant leisure. No endowment would render brilliant discoveries certain, so that fruitlessness might find ready apologies. This is a somewhat extreme statement of the case, though not a wholly unjust one. Such forebodings as these might be realized, although it does not to me seem probable that the men, who now labor so earnestly and assiduously for the love of knowledge, would accomplish any less because of greater encouragement. Still, an endowment which afforded large salaries for no regular, specific duties, might in some cases work mischief. The true man of science would hardly advise even a possible wasting of means.

The idea of an endowed laboratory for research is of a very different kind. It proposes an institution in which salaries shall be paid for work of a definite, prescribed character. Men would not be employed in it to make showy discoveries, although these might be rendered incidentally possible. The laboratory would, in short, be a place wherein the fundamental data of chemistry and physics should be accurately established, without more than casual reference to particular industrial questions or to theories. In it a body of trained specialists would coöperate upon such researches as I have suggested; they would fix some of the foundation stones of science; they would combine their strength upon those tasks which are too large and too arduous for individuals to undertake. Just as an astronomer makes an exact map of a given zone in the heavens, observing the position of each star

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over and over again; so the workers in this laboratory would undertake repeatedly those delicate measurements which physical science so much needs. The physical properties of substances could be exactly determined; methods of measurement could be tested and improved; apparatus too expensive for individuals to own might be constructed and employed; the quantitative relations of the several forces could be accurately ascertained, One great problem at a time might be taken up, and the energy of the whole body of workers concentrated upon it. Under ordinary existing circumstances a scientific man teaches several hours a day, and after that he labors at research. Here he would have a different routine from that of teaching, and probably as much leisure for himself and his own ideas as before. Indeed, to carry out the latter his opportunities would be vastly improved.

Who can doubt that such a laboratory would benefit science? To have important data systematically determined, with every conceivable precaution and the best appliances, would certainly be worth while. Every industry involving applications of physics or chemistry would be aided. The special problems of the iron-master might not be touched, and yet evidence would be discovered which should enable him to solve them much more easily and certainly than before. Precision would be gained everywhere; in facts, in methods, and in appliances; theories would rest on more solid foundations; the intellect would find new food for thought. The work done might seem at first sight to be of the hardest, dryest, and most practical character; and yet there would be within it more than utility. We cannot deepen our knowledge of the real, without at the same time broadening our insight into the ideal. The more we know, the vaster the unknown seems to us. The better we know, the more truly do we recognize the imperfections of our knowledge. We climb each mountain only to see a bluer, higher, grander summit far beyond.

Can this conception of a laboratory for pure research be realized? I believe it can. Physics and chemistry have created millions upon millions of wealth; they have helped to bring distant countries nearer together; they have benefited every human being within the limits of civilization. In every country of the civilized world there are many rich men who owe their wealth to the applications of these sciences. In our own country especially, we find such men ready and often anxious to give. Every day we

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hear of large bequests for public purposes; of gifts to colleges, libraries, observatories and museums; to hospitals, asylums and churches; to the poor and to the heathen. Surely some of the wealth which our science has rendered possible, ought to come back a little more directly for her benefit. There must be men who would give liberally to her purposes if they only knew of her needs. Can we not do something towards making our wants known? Are we not able to present a strong case in our own favor? Might we not by some combined effort at least make a beginning, and secure a fund, which should serve as a fulcrum for our future exertions? Let a nucleus once be established near some large university, or under the control of such an institution as the Smithsonian, and it cannot fail to grow. If the first organized laboratory, for such work as I propose, could be founded in this country, it would soon be recognized as a national glory and a benefactor of the whole human race. I throw out these suggestions merely as suggestions, with the hope that some means may speedily be found which shall lead us to the much desired end.

In Europe the needs of science are often liberally provided for by Government. The scientific institutions of Germany, for example, owe their existence and support chiefly to funds drawn from the public treasury. This policy I believe to be wise and proper; although it might not be judicious to urge it strongly here. With us, the proposal to establish a purely scientific institution, upon moneys raised by general taxation, would undoubtedly be seriously opposed. The public importance of the measure would be obscured by a clamor about economy, and upon the wickedness of expending the means of the many for the benefit of the scientific few. Much of this opposition would be foolish, although at the same time effective. Still, granting that it might be unwise to ask the National Government to establish a laboratory for research, there is another kind of laboratory which it might build, equip and organize, with unquestionable propriety.

All the time, the General Government finds it necessary to employ chemists for special expert work. For the Bureau of Engraving and Printing, investigations are made with reference to the inks and paper used in the manufacture of notes and bonds. Questions are continually arising in connection with the custom houses, which can only be settled by the testimony of a reliable chemist. Frequently, the Examiners of Patents would be much

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aided in their decisions if they had laboratory facilities at their command. The War and Navy departments are continually needing work done, it may be in the examination of supplies, in testing metals, or in experiments upon explosives. For the Light-House Board there are oils to be tested, and for the Geological Surveys there are rocks and minerals to be analyzed. As for the Agricultural Department, it already has a small laboratory, and there is another connected with the Army Medical Museum. These examples do not by any means complete the list; they only serve to show how great an amount of scientific labor the National Government is obliged annually to employ. The work is now done in a scattered way, often with imperfect appliances, and much less thoroughly than is desirable. The question I now raise is this: Would it not be eminently proper for the Government to establish a first-class chemical and physical laboratory, in which all of its work in these departments should be done by an adequate body of scientific men, provided with the best materials and apparatus? Would not this be a measure of true public economy? Could not more and better work be done for a given sum of money than is done now? To me it seems that the plan is decidedly practical, and that Congress might by judicious pressure be induced to carry it out. Of course, the measure is open to the usual political objections. It might enable inefficient men to get responsible positions through political influence; and there would be a possible danger that corruption should secure another foothold where it is already too firmly lodged. This objection I do not believe to be sound. At present, experts may be employed through political favor, while the country at large knows nothing of what is going on. In a national laboratory the appointments would be conspicuous, and mismanagement would at once be recognized. There is an argument from analogy which may be even stronger than this. Whatever weakness our public offices in general may have exhibited, our scientific service has always been remarkably able and strong. If the proposed laboratory could be made as efficient as the Naval Observatory, the Coast Survey, or the Army Medical Museum; if it could secure as thorough men as these have secured, the objections just urged would disappear. The nation must get the work done somehow, and would it not be best done in the manner I propose?

Such a laboratory as this, organized at first for the merely prac-

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tical work of the Government, might in course of time widen its scope a little. The National Observatory, established originally for the purposes of the Navy Department, has been able to do much for science beyond its purely routine labors. The expenditure of public money for eclipse and transit expeditions, in furnishing the means for work like that of Newcomb and Hall, has been for the interests of abstract science; but I doubt whether any intelligent citizen has ever begrudged a dollar of it. The nation has been ennobled by such researches, even though no material benefits should ever spring from them. So with a national laboratory. At first, its work would be only a technical routine; but sooner or later this would necessarily develop into something more. The Government investigations could not be carried on forever by imperfect methods; and, accordingly, better methods would have to be devised. Whatever researches tended towards improving the work done for the nation would, of course, be properly carried out in a national laboratory. Thus an institution, established for practical purposes at public expense, could at the same time benefit the public service, and advance the interests of science. The cost would be justified over and over again in a purely material way, so that the question of the usefulness of the laboratory could never be seriously raised.

As we are gathered together for the Advancement of Science, I may be permitted to broach one more subject which I believe to be of some importance. Year after year the researches of American chemists are steadily increasing in number, extent and value. Work like that of Gibbs, Cooke, Smith, Mallet, Remsen, Jackson, and a dozen others who might be named, is certainly work of which we may reasonably feel proud. But how is all this material published? A little of it in the American Journal of Science and Arts; a part in certain foreign periodicals; another portion in several local transactions, as for example, the Proceedings of the American Academy. In short, the work is widely scattered; and some of it is effectually buried beyond the reach of a majority of our fellow chemists. We need, I believe, in this country, a good chemical journal which shall fairly represent the work that America is doing for chemistry. A journal, in which every chemical research published in the United States should find a place, either in full or in abstract, would certainly stimulate investigation among us, and send all over the world a truer estimate of our

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scientific standing. We might have for our organ a chemical periodical equal to any in the world; and I sincerely hope that we may not have to wait long for its establishment. Where or by whom it should be edited and published, is a question I will not attempt to raise; suffice it to say that it ought to be controlled by no local clique, and managed in no Pharisaical spirit. It should be broad enough not to fear novelty, and courageous enough to reject trash; two qualifications more easily stated than found. I throw out these suggestions, hoping that they may take root somewhere, and that without much delay. Even though we ourselves may lack the power to carry them out, our recognition of their importance may lead to action elsewhere. If we in our gatherings can point out good pathways, others with richer means may be induced to follow them. Our united counsels and combined efforts may achieve results where, working separately, we should fail.

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# PAPERS READ.

A SHORT ACCOUNT OF THE NATURE OF THE OXIDE OF THE NEW ELEMENT MOSANDRUM. By J. LAWRENCE SMITH, Louisville, Ky.

THIS element, which I have thought proper to call *Mosandrum* in honor of the important labors of the distinguished chemist Mosander, in connection with this class of earths, was discovered by me in the latter part of 1876, and at the meeting of Academy of Natural Sciences, May 8, 1877, I made a communication on the subject, as will be found in the proceedings of that date. It was discovered in the Samarskite of North Carolina, which has been found in considerable quantity. In my analysis of this mineral, and the examination of the earths contained in it, I was surprised not to find cerium oxide among them; or, if present, only as a mere trace. After the earths are separated and purified, then converted into a nitrate, and evaporated to a syrupy consistency, a concentrated solution of sulphate of potash is added with a large excess of crystals of same salts; this is stirred several times and allowed to stand for twelve or eighteen hours, when a large deposit of a double salt takes place, which is thrown on a filter and washed with a concentrated solution of sulphate of potash.

This double salt is now dissolved in water acidulated with chlor-hydric acid, and heated; subsequently neutralized, and then precipitated by oxalic acid: this oxalate is washed and ignited, and the oxide resulting therefrom redissolved in nitric acid and reprecipitated to get rid of potash contained in the first oxalate. It is in the oxide (*A*) thus prepared, that the Mosandrum oxide will be found. Some of it is in the washings by the solution of the potash sulphate, and in the first mother water, for its double potash sulphate is slightly soluble in a concentrated solution of potash sulphate, placing it in this respect intermediate between the oxides of cerium and yttrium; which, as the results of my labors, in connection with those of Marignac and Delafontaine demonstrate, run so insensibly into each other, that it is hard to find the line of demarcation between the groups.



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The oxide (*A*) has the following properties: A chrome yellow color of medium intensity; slowly soluble in very dilute solution of nitric acid (1 to 100), but completely in from twenty to thirty minutes; precipitated readily and completely by oxalic acid; and this oxalate at a red heat is readily converted into oxide without blackening, as is the case to a slight extent with the oxalate of lanthanum; a concentrated solution of sulphate of potash, with the addition of some crystals of the same salt poured on a concentrated solution of the nitrate, will gradually furnish an abundant precipitate very slightly soluble in a concentrated solution of the potash salt. The sulphate is not very soluble and can be obtained in small crystals; the nitrate is very soluble and furnishes large prismatic crystals from a very concentrated solution, the salts are permanent in dry air, but deliquesce if the air be moist; the salts are of a slightly pinkish color, but this is due to the presence of a little didymium; in the absence of this last metal, the salts are colorless—a solution of this salt (1 in 10 or 20 of water), and a centimeter thick, gives didymium absorption line in the bright part of the spectrum.

If this oxide be dissolved and then precipitated by potash, we obtain a gelatinous mass, which, when treated with three or four times its weight of caustic potash, or soda dissolved in a little water and then subjected to a current of chlorine for two or three hours, is all dissolved but a small amount of the precipitate, rendering the solution slightly milky; the whole thrown upon a filter leaves a white gelatinous precipitate representing less than one per cent. of the mineral, not having the appearance of cerium oxide, but more nearly resembling alumina; it is readily soluble in the strong acids, and when converted into a sulphate is easily dissolved in cold water, which on heating deposits a cobweb-like flocculent precipitate with all the characteristics of sulphate of thoria.

In fact, the above new method of separating thoria is all that can be desired, as I have subsequently proved by experimenting with pure thoria.

The trace of cerium present will go with the thoria, but this can only be detected when operating on a large quantity of the mineral.

After separating the thoria with chlorine gas, the soluble oxides are acidulated, and precipitated twice by oxalic acid. It still con-

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tains the didymium, and this by fractional precipitation with ammonia (in a manner which I shall describe in detail in a forthcoming paper on the analysis of the Columbates and Tantalates) enables me to condense the didymium in the last precipitate.

I reduce the didymium present in the oxide to one or two per cent. without much difficulty; to get rid of it completely is more tedious.

The oxide (*A*) thus freed from thoria and didymium oxide is of a bright chrome yellow color and differs from the analogous oxides, the existence of which is recognized by chemists.

1st. From cerium oxide by complete solubility in dilute nitric acid, by its solubility in caustic potash supersaturated by chlorine gas, by the greater solubility of its double potash sulphate in a concentrated solution of potash, etc.

2d. From lanthanium oxide by the color of the oxides; by the manner of the combustion of the oxalate, never carbonizing in any part, which the lanthanium oxalate commonly does, and complete combustion of its oxalate furnishing no carbonate as the lanthanium oxide does; the same relative difference in regard to the double potash sulphate, as in the case of the cerium oxide; testing fractional precipitates of the oxalate only confirmed still further this difference, furnishing no evidence even of the presence of lanthanium oxide.

3d. From didymium oxide by the color and other properties of the oxide and salts; also similar ground of difference mentioned under the two previous heads; but under this head nothing further is necessary to determine the difference than the peculiar absorption bands of didymium in the bright part of the spectrum, the new earth not furnishing any bands. As the earth is first obtained, the didymium bands are always visible, but this impurity has been reduced to about one per cent., only the very faintest shadow of a band being seen in a solution of the oxide (1 to 20), one centimeter thick.

4th. From erbium oxide by its color (erbia being of a very light rose color), and by the more ready solubility of the double potash sulphate solution, and by other chemical properties that are useless to mention, as both the luminous rays and absorption bands serve to distinguish erbia from this and all other earths.

5th. From yttria by the color of the oxide (yttria being nearly perfectly white), and, by being less readily soluble in the strong



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acid. The most marked difference yet noted is with the double potash sulphate, that of yttria being comparatively readily soluble in the potash sulphate, while the oxide of Mosandrum is almost as insoluble as the sulphate of cerium and potash.

The only earth left with which to contrast it was the hypothetical terbia of Mosander, and so far as I could compare its properties with those that the supposed terbia was said to possess they were not the same; and besides, the existence of this earth has been contested by M. M. Bahr and Bunsen, and by M. M. Cleve and Hoeglund. I therefore felt warranted in pronouncing it a new earth, and so stated it to the Philadelphia Academy of Sciences, in May, 1877, and in a sealed package to the French Academy of Sciences, Sept. 22, 1877.

But I was not yet prepared to name the earth, and in the month of March, 1878 (Archives des Science Phys. et Nat., 15 Mars, 1878), I read two communications on the subject of terbia, one by M. Delafontaine and one by M. Marignac. I sent some of the new earth and its nitrate to M. Marignac, desiring to obtain a specimen of his terbia, upon which he had been working, this last he was not able to do, as he possessed as yet too small a quantity of the terbia.

A little before the arrival of the earth to the hands of M. Marignac, M. Soret, of Geneva, had been experimenting with the rays and absorption bands of various substances on the extra violet rays, in a manner pointed out in the publication of his researches,[1] and published the results obtained by experimenting with the earths from Gadolinite,[2] especially upon the terbia of M. Marignac. In these he obtained a new set of lines that could only be attributed to a new earth and not to terbia, which last he considers as having no absorption bands in the luminous or ultra violet part of the spectrum.

M. Marignac was kind enough to get M. Soret to submit the earths, which I had sent him, to the same spectroscopic examination, the result of which was to find that the earth corresponded exactly with his, spectroscopically, giving the terbia without absorption band, mixed with an earth with the absorption bands of No. 2 in printed diagram accompanying his investigations on the

[1] Archiv. Science Phys. et Nat., Geneva, March, 1878, p. 322. Comp. Rend. de l'Acad. Science, Paris, March, 1878, p. 708.

[2] Compt. Rend. de l'Acad. Science, Paris, April, 1878, p. 1064.

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Gadolinite earth. This spectroscopic examination confirming what I supposed I had established more than a year ago, I on chemical ground have no longer delayed in giving to the new earth the name Mosandra, or oxide of Mosandrum, in honor of the distinguished chemist whose name is so intimately associated with the chemical researches on this class of earths. Much yet remains to be done to separate completely the Mosandra and terbia, but as I have an abundance of the earth it reduces itself to a matter of time and perseverance.

It is an interesting fact to note in regard to what are called the cerium and yttrium groups of earths, that they pass insensibly into each other, in relation to their several properties. If we take the action of concentrated potash and soda sulphates, yttria and ceria are at the two ends of the groups, and terbia and Mosandra in the middle; if we take their spectroscopic properties we find didymia at one end of the groups, Mosandra in the middle, and terbia at the other end, giving absorption bands. In fact, it seems to need these two elements, terbium and Mosandrum, to complete the groups.

Some matters of an historical interest in connection with this discovery can be found in the *Paquet Cachete*, opened at the French Academy of Science, July 22, and in a note following the same.[3]

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## The New Meteoric Mineral Daubréelite proved to be $Cr S^3 + Fe S$, and its frequent if not universal occurrence in Meteoric Irons.

By J. Lawrence Smith, Louisville, Ky.

When I first announced the discovery of this mineral the amount at my disposal was only sufficient to determine its specific characteristics; since then I have made numerous sections of the first iron which weighed about 250 kilograms, and also sections of another iron of 200 kilograms, from same locality (Cohahuila), and in this last have found the nodules even more abundant than in the first. Of the second iron I have a section with two polished surfaces of about 900 square centimeters each, and showing on

[3] Owing to Dr. Smith's absence in Europe, these two papers are printed without his revision. — Editor.

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the two surfaces twenty-five to thirty nodules varying from three to sixteen millimetres in diameter, at least ten of which are from one to one and one-half centimeters in diameter, and all of them exhibiting to the eye daubréelite in angular segregations.

The mineral first used for analysis was obtained by breaking it out from the nodules mixed with troilite and other impurities, depending on the eye to separate the impurities; since I find that chlor-hydric or fluor-hydric acid will attack the troilite readily and does not act on the daubréelite, so a method has been adopted by which the mineral has been obtained more abundantly and quite pure. The shavings and cuttings procured in making the sections were used (several kilograms of which were at my command); the fragments of iron were separated by a large magnet, and the small particles left behind consisted essentially of troilite and daubréelite; for the former is only feebly magnetic, and the latter not at all magnetic. Strong chlor-hydric acid is now added to this last portion, and gently warmed over a water bath. The troilite is readily attacked. After a time the first acid is poured off and a fresh portion added with the digestion continued over a water bath for one or two hours. The residue contains a good deal of light black matter that is easily washed away (this last has not yet been thoroughly examined, although much of it is impalpable daubréelite). The larger black particles are again treated with a little chlor-hydric acid, after which the daubréelite is left quite pure, and is easily washed and any foreign particles picked out.

In this form it consists, as already described, of shining black fragments more or less scaly in structure, not altogether unlike fine particles of molybdenite.

The fracture is uneven except in one direction, when there appears to be a cleavage. It is brittle and easily pulverized, the fine particles retaining their brilliancy. It is not magnetic; before the blow-pipe it undergoes but little alteration, losing its lustre but not fusing; and, after heating in the reducing flame, is slightly magnetic.

With borax it fuses slowly, the smallest particles giving an intense green color to the bead when cold. It is not acted upon in the slightest degree by chlor-hydric acid, either cold or hot, but dissolves slowly and completely in nitric acid when heated over a water bath, without, however, any liberation of free sulphur.

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Its specific gravity is 5·01. It is needless to give the details of the method of analysis. I will only take occasion to remark that when a mixture of hydrated oxides of chrome and iron are separated by the addition of bromine to an alkaline solution holding the oxides in suspension, that the operation must be repeated two or three times to ensure complete conversion of all the oxides of chrome into chromic acid, and consequently to separate it totally from the iron.

The following is an average of three analyses giving concordant results within one-half per cent of each constituent:—

| | |
|---|---|
| Sulphur, . . . . | 42·69 |
| Chrome, . . . . | 35·91 |
| Iron, . . . . | 20·10 |
| | 98·70 |

A minute quantity of what appeared to be a carbonaceous matter was mixed with the residual traces of oxides found in the mother water. It is very evident from the above proportions that this mineral is a sulphuret, corresponding in atomic constitution to the well known oxide chromite ($FeO + CrO^3$), daubréelite being $FeS + CrS^3$, the percentage of which last compound is

| | Calculated. | Found. |
|---|---|---|
| Sulphur, | 44·29 | 43·26 |
| Chrome, | 36·33 | 36·38 |
| Iron, | 19·38 | 20·36 |
| | 100·00 | 100·00 |

| | Calculated. | Found. |
|---|---|---|
| Sesquisulphuret of Chrome, | 69·55 | 68·00 |
| Sulphuret of Iron, | 30·45 | 29·75 |

The calculation of the daubréelite is based upon the sulphur found in the analyses (43·26). As yet we do not know of any terrestrial mineral corresponding to this, and it is an interesting fact that we are already enabled to establish so clearly its true composition, and also to obtain good characteristic specimens that will find their way into the principal cabinets of meteorites.

The occurrence of this mineral in so marked a manner in the Butcher meteoric irons of Cohahuila, when it does not show itself in the troilite of other meteoric irons, induced me to investigate

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the matter carefully, as I now had chemical methods to aid me. As yet I have only examined the troilite from three meteoric irons, viz.: those from Toluca, Sevier Co., Tenn., and Cranbourne, Austrailia; and in all three specimens it was found in marked quantities using about 2·5 grammes of troilite except in the case of the Cranbourne, where the quantity did not amount to one gramme, and the daubréelite was proportionally less than in the other two, the Toluca troilite furnishing the largest quantity.

The residue from 2·8 grammes of Toluca troilite after thorough treatment with chlor-hydric acid, which dissolves nearly the whole of it, was dissolved in part by nitric acid, and on analysis the solution was found to contain chrome and iron, representing about sixty milligrams of daubréelite. This mineral as obtained from these troilites was one of the pulverulent variety.

There is reason to believe that farther researches will show the constant presence of daubréelite in meteorites, and I am now prosecuting a series of experiments on the mineral segregations in meteoric irons, both those visible and invisible to the naked eye, and only discernible by chemical means which will tend to a satisfactory solution of this hypothesis.

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NOTES ON ANTIMONY TANNATE. By ELLEN SWALLOW RICHARDS and ALICE W. PALMER of Boston, Mass.

IN the course of some work on the determination of tannic acid, we tried Gerland's method of direct estimation by means of a standard solution of tartar emetic in presence of ammonium chloride. Gerland's formula (Fresenius' Zeitschrift, 1863, II, p. 419), is given as:—

$SbO_3(C_{18}H_8O_{12})_3$—or in the new nomenclature

$Sb_2O_3(C_{18}H_{16}O_{12})_3$ which requires

Sb 15·60 p. c. C 41·43 p. c. H 3·07 p. c.

The formula, that we have been led to adopt, is:—

$Sb_2(C_{14}H_8O_9)_3 + 6H_2O$ which requires

Sb 18·59 p. c. C 38·41 p. c. H 2·74 p. c.

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in which tannic acid is considered as di-gallic acid (H. Schiff, Bulletin de la Société Chimique [2], 16, p. 198) with possibly three phenol H's replaced by Sb, as well as the three acid H's. This formula is deduced from the following analyses of Antimony tannate. All the tannates described in this paper were prepared in the same manner. The solution, containing five to ten grammes of tannic acid in a litre, was heated to 60° Centigrade in a water-bath; tartar emetic solution (containing about ten grammes of the salt to the litre) was added, and then thirty cubic centimetres of ammonium acetate per litre. After the whole was shaken and allowed to settle, it was filtered and dried at 100° to 105° C. in an ordinary air-bath. The bulky, yellowish-white, gelatinous precipitate at first formed, became, when dry, yellowish to reddish-brown, transparent, amorphous, and broken into small, angular fragments.

*Antimony Tannates.*

| From: | C. | H. | Sb. |
|---|---|---|---|
| Tannin unfiltered (commercial), | 37·95 p. c. | 2·71 p. c. | |
| " filtered " | 39·30 | 2·86 | |
| " purified (by extraction | 37·38 | 2·91 | |
| with aqueous | 37·53 | 2·98 | 20·6 p. c. |
| ether) | 37·51 | 2·78 | |
| Nut-galls, | 38·32 | 2·88 | |
| | 38·47 | 2·81 | 20·5 |
| | 38·14 | 2·92 | |
| Sumac, No. I, | 39·60 | 2·70 | |
| " No. II, | 39·54 | 2·74 | |
| " No. III, | 40·51 | 2·92 | 20·1 |

The antimony was estimated as a sulphide, and the results are probably a little too high.

As to the process of titration, our first experience coincided with the statements of Gaulhe (Fresenius' Zeitschrift, 1863, III, p. 122), that the end of the reaction was difficult to seize and that the dilute solutions remained turbid. Even after we found an indicator, the process in our hands gave results varying with the amount of dilution, and the quantity of ammonium chloride employed. We then made a series of tests with other substances, viz.: alum, salts of sodium, etc., as precipitating agents. The precipitant chosen as a result of these tests was ammonium acetate, prepared by

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mixing in right proportion glacial acetic acid (containing 95 p. c. $C_2H_4O_2$) and ammonia water (containing 27 p. c. $NH_3$), so that one litre of the solution contained nearly 600 grammes of $NH^4C^2H^3O^2$. One cubic centimetre of this preparation, added for every twenty-five or thirty cubic centimetres of the total bulk, will give a clear supernatant liquid after standing for a few minutes. Without giving in detail the steps of the investigation, we give our process as we now use it.

The weighed quantity of the substance in which the tannic acid is to be estimated is taken in sufficient quantity to allow of at least three aliquot parts, each portion of 50 to 100 c. c. containing from ·100 to ·300 grammes of tannic acid. After the solution, obtained by digestion with water, is made up to a known bulk, three or four portions are measured out, and set in a water-bath to be heated to 50° or 60° C. The standard solution of the tartar emetic contains 6·730 grammes per litre of the salt ($KSbC^4H^4O^7$) dried at 100° C. One c. c. is considered to correspond to ·010 of a gramme of tannic acid. Gerland's formula would require 5·222 grammes per litre for the same value.

The estimation is facilitated by obtaining a maximum point and a minimum point at the first reading, as one portion is settling while the other is being treated; therefore tartar emetic is added from a burette to one portion in excess of the probable quantity required, and to another in less amount. The antimony tannate is then precipitated by the requisite amount of ammonium acetate and allowed to settle. A drop of the clear liquid is added to a drop of sodium hyposulphite on a porcelain plate heated gently to about 90° C. If the tartar emetic has been added in excess, the deep orange color of the antimony sulphide will at once appear. When this point is reached by successive additions of the standard solution to the minimum portion, we add to a third portion the estimated quantity, and test the clear liquid as a check on the loss occasioned by taking out several drops. We have found it easier to carry the titration to a decided orange tint, and to subtract 0·5 c. c. of tartar emetic solution for 100 c. c. of liquid, rather than to seize the first faint tinge, as most of the substances to be titrated contain coloring matters which give a yellowish or reddish tint, but not an orange color.

The next point of interest was to ascertain whether the method of titration, as given above, was applicable to tannin-holding sub-

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stances other than nut-galls and sumac. The following tests were made for this purpose:—

| | Tannin. |
|---|---|
| Leaves of sweet fern (comptonia asplenifolia) from near Boston, gathered the middle of May, | 7·56 p. c. |
| The same, gathered on the Kennebec River, Maine, the last of July, | 8·00 |
| Sample of ground hemlock-bark from Vermont, | 7·07 |
| Sample of catechu, | 29·70 |
| Sample of kino, | 41·50 |
| Crushed quercitron bark, | 7·00 |
| Congo tea, | 4·60 |
| Cinchona flava, | 9·60 |
| Ground cloves, | 7·03 |
| Hemlock bark from Maine, | 5·50 |
| Chestnut-oak bark from Careyville, Tennessee, | 3·00 |

We also prepared a quantity of antimony tannate from each of these substances in the same manner as we had prepared it from commercial tannin and sumac. The composition is given as follows:—

| | Sb. | C. | H. |
|---|---|---|---|
| Sweet fern, May, | 15·3 p. c. | 45·09 p. c. | 3·40 p. c. |
| Sweet fern, July, | 15.06 | 44·90 | 3·90 |
| Quercitron, | 12·8 | 49·50 | 3·42 |
| Chestnut-oak, | 15·5 | 47·51 | 3·63 |
| Cloves, | 12·5 | 43·30 | 3·09 |
| Hemlock bark, No. I, | 13·6 | 51·02 | 3·85 |
| Hemlock bark, No. II, | 13·5 | 49·86 | |
| Catechu, | 13·7 | 51·13 | 3·84 |
| Kino, | 15· | 50·71 | 3·72 |
| Cinchona flava, | 11·20 | 53·56 | 4·56 |
| Congo tea, | 11·40 | 47·30 | 4·00 |

These analyses showed that the composition of the precipitate was influenced by one of two causes; either the formula of the so-called tannin which united with the antimony contained more C and H than di-gallic acid, that is, it must be something like $SbOC_{38}H_{33}O_{17}$ or $Sb_2(C_{25}H_{21}O_{12})_3$; or the antimony tannate which was formed in the solution acted as a mordant, and carried down with it coloring matters which might or might not affect the titration, but which did affect the combustion.

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To determine how far this latter cause could be held responsible, we prepared antimony tannate from the sample of tannin which we used for all our experiments, and, having washed by decantation so as to keep the gelatinous precipitate in the best condition for absorbing color, we treated solutions of several of these substances with a quantity of antimony tannate corresponding to the estimated quantity of tannin contained in the solution, so as to have the conditions the same as in the previous precipitations; in one case, of the hemlock, we varied the proportions as noted. The average composition of antimony tannate, as we have already prepared it from tannin, sumac and nut-galls, is given at the end of the list for comparison.

| | Sb. | C. | H. |
|---|---|---|---|
| Antimony tannate + coloring matter of sweet fern, | 15·7 p. c. | 46·21 p. c. | 3·73 p. c. |
| " " + " " " quercitron, | 9·54 | 45·90 | 3·80 |
| " " + " " " hemlock, | | 63·30 | 3·60 |
| " " + " " " hemlock, | | | |
| (5 times as much antimony tannate having been taken), | 13·4 | 43·40 | 3·90 |
| Antimony tannate, | 20· | 38·21 | 2·86 |

In the case of sweet fern and quercitron the results are very nearly those obtained by direct precipitation with the tartar emetic solution in presence of ammonium acetate.

We were greatly surprised by the behavior of the solution of hemlock bark. In all cases, after the treatment of the solution by the previously prepared antimony tannate, we precipitated the remaining tannin by tartar emetic as usual, and noted the quantity required as compared with that required for the precipitation of the tannin in the original solution. In the case of hemlock, there was scarcely a trace of a precipitate, showing that the antimony tannate had dragged down, or united with, all the substance which had been supposed to be tannin.

In order further to test the characters of the supposed coloring matters in these substances, we made a series of trials with mordanted yarn. From hemlock, we obtained, on wool mordanted with tin chloride, and on cotton mordanted with alumina, a brown-red color; from sweet fern, in both cases, we obtained a brilliant yellow color, quite equal to that from quercitron. We then tested solutions of all the substances upon which we had been working, with cloth mordanted in the usual way for calico-printing (*i. e.*, with iron and alumina in alternate stripes), in order to show the presence of tannin and other coloring matter at the same time. Samples of the cloth thus dyed are shown on the accompanying card.

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The yellow in sweet fern seems closely allied to, if not identical with, the quercetin derived from oak-bark. A solution of sweet fern guarancined (boiled with very dilute sulphuric acid) behaves like a solution of quercitron bark, a black, gummy mass being formed, and the solution depositing yellow flakes which dye intensively.

Two pieces of cloth of equal size, dyed with equal quantities of sweet fern leaves and quercitron bark (a gramme in each case being taken) showed rather more tannin and less yellow for the sweet fern, and more yellow and less tannin for the quercitron. A single trial for the amount of yellow in sweet fern leaves, made by weighing the antimony tannate which had carried down the yellow with it, and which had been added in known quantity, gave 2·5 per cent and the amount of tannin in the filtrate had decreased about three of the eight per cent previously found. This indicates that the antimony combines with a portion of the coloring matter as well as with the tannin. This is further indicated by the fact that the quercetin-like color, obtained by guarancining, was precipitated by antimony, as also quercetin itself, prepared from bark. If this is so, the formula of a part of the color must be very near to that of digallic acid. Quercetin acid is given in Heppe's "Die Chemischen Reactionen" as $C_{15}H_{10}O_7$, and quercetin as $C_{27}H_{18}O_{12}$. These would give nearly the same per cents as those of digallic acid in our formula.

| | Sb. | C. | H. |
|---|---|---|---|
| $Sb_2(C_{27}H_{16}O_{12})_3 + 6H_2O$ Quercetin. | 12·52 p. c. | 50·0 p. c. | 3·08 p. c. |
| $Sb_2(C_{15}H_8O_7)_3 + 6H_2O$ Quercetin acid. | 19·49 | 43·13 | 2·87 |
| $Sb_2(C_{14}H_8O_9)_3 + 6H_2O$ | 18·59 | 38·41 | 2·74 |

On referring to the samples on the card, it will be seen that in all cases where a good black is obtained, a yellow coloring matter more or less intense, is found, which seems allied to, or identical with, the color of quercitron. Fustic seems to offer the only exception to this statement. This yellow is precipitated by tartar emetic, and thus the titration of these substances would give results too high. In the other substances tested, the reddish-brown or reddish-yellow color itself is precipitated by tartar emetic, and a large portion of that substance which gives the dark bands is found in the filtrate. As gallic acid is not precipitated by tartar

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emetic, it is possible that these substances contain gallic instead of di-gallic acid.

Gerland states that neither gallic acid nor the coloring matters affect his process, but our experiments do not confirm the latter statement.

Woman's Laboratory, Mass. Inst. Tech., Aug., 1878.

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ELECTRICITY AN AGENT IN VEGETABLE GROWTH. By A. P. S. STUART, of Lincoln, Nebraska.

GRANDEAU, the Director of one of the agricultural experimental stations of France, has recently made some very interesting experiments relative to the influence of atmospheric electricity on vegetable growth. From these experiments, it appears, that when plants, as for instance, tobacco, maize, or wheat, otherwise equally conditioned, are exposed to the influence of electricity, they grow far more luxuriantly, and assimilate a far greater proportion of nitrogen in some one or more of its numerous compounds, than when withdrawn from such influence. This latter fact, the assimilation of an unusual quantity of nitrogen under electric influence, is very interesting, as indicating in plant growth the possible and perhaps probable absorption of nitrogen directly from the atmosphere, a doctrine which has been stoutly advocated by some, and as vigorously denied by others, although, so far as is known to the writer, such absorption of nitrogen by plants directly from the atmosphere has not until recently been ascribed to the agency of electricity.

These experiments of Grandeau are in perfect harmony with, and indeed seem to be instructive illustrations of, the experiments of Berthelot, made only a year or two previously, by which he established the fact that organic matter, like dextrose, and even moistened paper, absorbs nitrogen directly from the atmosphere under the influence of electric tension, and forms under such circumstances a nitrogenous compound, which, when heated with soda-lime, evolves ammonia. To the careful observer, who may

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have noticed a sudden and wonderful increase of vegetation simultaneously with unusual electric manifestations, as during thunder-storms in spring-time, the thought may have occurred that electricity is in some way connected with vegetable growth. These experiments of Grandeau and Berthelot teach us that such a fact is not only possible, but certain; and we have called attention to them now, not only because they are exceedingly important, but because they remind us of a similar experiment made by the writer several years since. Suspecting that electricity was in some way concerned in plant growth, some beans, of as nearly equal size as could be obtained, were planted in pots and kept under as nearly equal conditions of soil, moisture, temperature, etc., as could well be, with this difference, that around the beans in one pot a coil of copper wire was arranged, and a feeble voltaic current made to pass continuously during germination and growth. The result was that the beans surrounded by the coil of wire grew much more luxuriantly than those in the other pot. At the time it was thought that possibly slight differences of condition, not understood, might be the cause of the difference of growth, and that further experiments would be necessary in order to pronounce with certainty that electricity was the chief or sole cause of the difference.

The experiments of Grandeau and Berthelot, as well as that just described, all unlike each other in conception and detail, point in their results, with almost unerring certainty, to the conclusion that electricity is a potent agent in vegetable growth. It may not be possible to say just how it acts. It is difficult to say just how electricity, in seeking an equilibrium in the form of a spark, induces oxygen and hydrogen in the jar, or oxygen and nitrogen in the atmosphere, to combine,—yet they are none the less facts because of our ignorance of the modus operandi; and it seems that the same agent is constantly exerting in the atmosphere a quiet, insensible, but none the less effective influence of the same kind, but varying in degree according to the electric condition of the atmosphere. In this light it is easy to see how exceedingly important this agent becomes in inducing those manifold chemical combinations, which, together, constitute what we call vegetable growth.

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## TITLES OF OTHER PAPERS READ IN SUBSECTION OF CHEMISTRY.

On the development of Ozone in the air by means of sudden changes of temperature. By Benjamin S. Hedrick, of Washington, D. C.

The Electrolytic Estimation of Mercury. By F. W. Clarke, of Cincinnati, Ohio.

Some New Selenocyanates. By F. W. Clarke., of Cincinnati, Ohio.

Some Specific Gravity determinations and conclusions from them. By F. W. Clarke, of Cincinnati, Ohio.

## TITLES OF PAPERS READ IN SUBSECTION OF MICROSCOPY.

On Recent Progress in Microscopy. By R. H. Ward, of Troy, N. Y.

On a Standard Inch and a Standard Centimetre. By William A. Rogers, of Cambridge, Mass. (This paper will appear in the Proceedings of the American Academy Science and Arts.)



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# SECTION B,

# NATURAL HISTORY.

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# ADDRESS

OF

# PROFESSOR AUGUSTUS R. GROTE,

VICE PRESIDENT, SECTION B.

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MEMBERS OF THE ASSOCIATION: —

LADIES AND GENTLEMEN: —

THE two Vice Presidential addresses of my predecessors are remembered for the honor they reflected upon American science. That of Professor Morse, at the Buffalo meeting, two years ago, had for its subject, "What American Scientists have done for Evolution." This address has a permanent historical value, which is only lessened through the modesty of its author, who forgot his own in enumerating the labors of others. That of Professor Marsh, at the Nashville meeting of last year, dealt with the "Introduction and Succession of Vertebrate Life in America," and its sketch of the progress of the higher forms of life, however the details may be filled out by subsequent researches, established the certainty of that development by succession as the result of the author's own enduring work. In this address I have endeavored to trace the action of evolution in a further stage, and, in speaking on "Education and the Succession of Experiences," to lay before you a brief study of the growth of the mind.

A study of the phenomena of the human intellect teaches us that there is a process going on by which external matters are



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being pictured in the brain through the action of the senses. The imagination itself is found to depend upon an ideal grouping of experiences, however fantastic and unconnected they may be made to appear. As time elapses and mankind advances, the brain pictures seem to become more perfect and to embrace more fully the characters of their originals, and their development in this direction is proved by our varying notions of things, and our changing conduct toward them. Education may, then, be primarily conceived as the process of storing sense impressions in the brain, and the total condition and amount of the brain-pictures we might style knowledge. An education seems to resolve itself at last into a succession of experiences, however complex it may appear in its results, so that, in any discussion, it will make the matter much clearer if we study the machinery which makes education itself at all possible. It is evident that we only appreciate sight, or any one other of our senses, because it exists; and, in the evolution of sense organs, the feeling of the want of them in the individual evidently succeeds their possession by the species. Through the senses a section of the universe is opened to us, a section which is limited by their powers in bringing, and the capacity of the brain for storing what is brought. But with the utmost exercise of our receptive powers our view of the world remains a section still. When we find, for instance, that vibrations of the air below thirty-two and above 100,000 per second make no impression on the human ear, we understand that we are cut off from a wide range of possible sound. So that every mechanical appliance by which we can enlarge the field of experience tends to increase our knowledge, and, therefore, to affect our conceptions of the world about us.

The "atoms" of the intellect are thus seen to be the single sense impressions, and the conclusion is inevitable that reason is generalized experience. It is possible to check the correctness of this conclusion through a study of the intelligence of the lower animals, and the efficiency of their senses. And so far we have found that the generalizations of knowledge which they are able to make, and which we have called in the past instinct, to distinguish them from our own reason, stand in direct relation to their capacity of receiving sense-impressions. It may be said that what the lower animals do know, they come to know by similar means, and in the same way that we acquire knowledge. It is

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now the generally accepted conclusion by scientific minds that instinct and reason differ in degree, and not in kind. Our literature is already full of proof, drawn from the habits of vertebrate and invertebrate animals, that this position is a just one, and explains fully the relationship between the intelligences of the different animals themselves, as well as between the intelligence of the races of men and that of lower types of existence. Reason is, then, built up out of past experience, which we use when brought into fresh contact with things. We become more reasonable as we experience more, and the object of education is to impart that reasonableness more quickly, so that the growing generation may not be obliged to find out how things are solely from the process of its own experience, but may profit by the knowledge which was gathered by those who passed away before. Reason, then, depending on the sense impressions, must be affected by the character of the sense organs. If these were more perfect the acquirement of knowledge would be easier. In fact, they are found to be limited and unreliable beyond what we might at first be inclined to grant. But however justly we may distrust our senses on any particular occasion, we cannot consider them, or any one of them, as totally unreliable. All that we can do is to point out their insufficiencies, to check the evidence of one sense by that of another, to try by repeated experiment to establish the actual condition of affairs.

The pictures which our senses give us are not to be regarded as false, although they may convey a misleading idea of the object perceived. Every action of the senses gives us a comparatively true perception of external objects. A correct judgment is formed after a full examination through all the senses that can be brought to bear on the subject and in this way the actual state of affairs may be discovered. The method of examination we may employ is determined not only by past experience, but by that analogy which prompts fresh experiments. At the bottom this is what makes education attractive, that the mind grows in the direction to receive more and more complete pictures of things. And this is an answer to those who object to scientific hypotheses, that all such, based on observation or sense-impressions, are to some extent reliable and serviceable. It is not true, either in part or wholly, that we "take leave of our senses" to construct our scientific hypothesis, but the reasonableness of our hypothesis stands in

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relation to the extent of our acquaintance with its subject and related matters. But aside from the imperfection of our senses, which may make a correct judgment impossible, or at least difficult, we find ourselves sometimes under exciting conditions of the brain, when we mistake the subject of our thought for the object of our sense-impressions. "False-seeing" and "false-hearing" have come to be classed among the diseases of the brain, and the evidence collected on this head points to the conclusion that it is the memory which is here affected. The explanation of memory is, that it is the result of an effort of the mind by which we re-collect the pictures and impressions already stored in the brain. The mind, "that veiled enchantress," as Draper calls her, is "veiled" because her feeders are so subtile that we believe she exists without subsistence. But in all her phases she is seen to depend literally upon the senses for her vigor. The dream-doors of night's dwelling, opening at the touch of sleep, let forth a troop of images seemingly of fresh import, and perhaps of direst consequence. But reason, by simply bringing the figures into their proper sequence, reveals in them the pictures of the day's experience.

Before discussing the imperfections of the senses, which so deeply affect our conceptions, let us briefly consider the way in which one of them, that of sight, receives its impressions and conveys them to the brain.

When we look at any object, our eyes are affected by undulations of the ether. The impact of these waves upon the extremities of the nerves of sight in the retina is real, and the common saying that we have been struck by some appearance is a true one. The reason that we do not apprehend the strictly material character of these manifestations lies in the delicacy of the organ of vision itself, and the slight movement of the ether. When, however, in the same manner, air is violently set in motion, as in wind, it strikes against our whole body and we recognize it by the less delicate sense of touch. In the eye, the rapid light waves strike upon the retina or sensitive portion of that organ, and the action which is set up there is communicated to the fibres of the nerves of sight, and conveyed by them to the brain. When this action reaches the brain, we see, but the simple working of the mechanism of the eye, without reaching the brain, would leave us blind.

Young, in his theory of the perception of color, has shown that there are three orders of pointlets or nerve ends of the retina, and

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that each order of nerve ends is sensitive to a different rapidity of the light waves. Helmholtz demonstrates that certain of the nerve ends in the retina are destructively affected, in both the live and the dead subject, by particular rays of light. We may conclude that the shape of any object, as it appears to us, depends upon the number of pointlets of the retina affected by it, and the color on the order of nerve ends stimulated by it so far as our sight of the object is concerned. The cause of the color of any particular object itself must be sought in the structure of its atoms, which absorb certain of the rays of light and reflect others. Thus, all objects may be considered, in a general way, prismatic; that is, they effect a separation of the rays of white light in displaying the phenomena of color, but they do this by absorbing certain of the rays. This is the point where the chemist and the biologist meet,—every phenomenon displayed by an object is found to depend upon the ultimate structure and arrangement of its component atoms. We may thus conceive of the action of the sense of sight without calling to our aid anything but the thing seen, the medium through which the thing attacks the eye, and the machinery which conveys the picture of the retina to the brain where it is registered. We may in this way investigate the action of the remaining senses, and with a similar result. Always there is the thing perceived, the media, the contact, the machinery of the sensory nerves, and the gray matter of the brain. Defining education, then, as the process of receiving a series of sense impressions and experiences, let us consider in what way our knowledge is affected by the limitation and imperfection of the senses.

We have said that, from the limitation and imperfection of our senses, but a section of the universe is opened to us; in reality, we have but a partial idea of even this section. It will occur to every one that there are a large number of objects of which they fail to take cognizance, although they could do so if they had the opportunity. Such objects as distant portions of the earth's surface, foreign animals and plants, many of us have to content ourselves without seeing. We know them through the senses of other persons, who have written about them. Nevertheless, our total conception of the world is affected by our personal ignorance of these matters which go to make up the world. But, aside from this class of objects, there are also to be considered the large number of things we pass over from inattention and a want of

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preparation of the senses to entertain them. How large a class this is we can easily understand, when we know that the difference between scientists and other people is involved in the matter. For a scientist is merely one who diligently observes things that for the most part are neither seen, nor heard, nor felt by others. Life is also too short to witness the outcome of many things, and the years in which we are willing to pursue inquiry too few. It has happened that generations have elapsed before a new age has carried on seriously the investigations commenced at an earlier epoch. The eternal appearing and disappearing of things, of which we are a part, it requires all our courage and genius to face and understand.

And then, as to the limitations of the senses, we know from the microscope that there are quantities of forms too small for us to know without artificial help, and even correctly with it. We do not recognize objects at a certain distance, and our power of vision is probably surpassed in this respect by hawks and eagles. In similar ways, all our senses have their limitations, which it is the object of art and science to attack in the interest of human advancement. Aside from their limitation as to power, our senses are apt to deceive us by taking only a particular side of the object, one view, as it were, from which alone we must receive a permanently incomplete, and therefore incorrect, idea. Again the same sense obtains different results under different conditions, as when Pythagoras observed that a stick placed in the water appeared to be broken. We contrast the picture of the stick in the water with its picture in the air, and although both pictures are seen to be correct, we test the truth by the sense of touch, and arrive at the conclusion that the stick only *appears* to be broken in the water. This illustrates what I have said before as to our testing the results of any observation by different senses. In this case this testing gave us the theory of the refraction of light and thus enlarged our conceptions of the properties of matter. In an address before the Russian Entomological Society, to which I have only space to allude, Von Baer most interestingly shows how our total conception of time itself is modified by the rate of the circulation of the blood. It is only another instance of the dependence of our conceptions upon our constitution. So far as we are concerned, we have reached, not the ideal, but the final truth, whenever we have exhausted our powers of perception in

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any given direction and by a systematized checking of results, arrived at a conclusion. The duty of accepting this conclusion and acting upon it, seems to me imperative and is what science demands. Anything short of this is a practical denial of the value of all knowledge, which would lead to the disintegration of society. I hope to show, further on, that science assists the development of the whole State, and adds to the stability of government by publishing its conclusions, and wish to insist here that it is fundamentally beneficial to the same interests in making its observations. But from this imperfection and limitation of our senses comes, not only a succession of experiences which are incomplete, but a general concept with regard to external matters, which must be of necessity misleading. We are here but a short time, and see little of the outcome of passing events, and can know nothing of the outcome of the world itself. Thus it has come to pass that what we have not fully observed, we have assigned to an unknown cause. We have fitted nature into our own measure, directly led thereto by the imperfection of our knowledge, and we have arrived at the concept that design exists in the world about us as it seems to us to be displayed in our own handiwork and in the work of animals. But in reality what we see in the details of the structure of animals and plants at least is not design, but adaptation. Suppose we leave a coat in a closet, and while it is there it is visited by a female clothes-moth, which deposits thereon numerous eggs. The little worms hatched from the eggs would at once commence to make free with the nap, and eat holes in the coat with a good appetite. If they ever thought about the matter, would they not conclude that the coat was hung there for their special benefit? They would do so merely because the coat was there. The fact that they adapted it to their own use would be construed by them into a belief that it was designed for their benefit. They would inevitably regard the owner of the coat, could they arrive at this conception, as their benefactor and the preserver of the whole race of maggots. They would know nothing of the thousands of clothes-worms that perish because they never get anything to eat. The fact that life is sacrificed by the wholesale in nature tells against the argument of design. And nature is as careless of the species as of the individual. In the

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crust of the earth are contained the remains of thousands of types of form of which nature has not been careful, but has crushed them out, because they could not adapt themselves to the changing conditions which surrounded them. And just as the human body displays an incomplete adaptation as seen in our possession of useless parts, such as the vermiform appendage, which is present also in the anthropoid apes, so our sense organs are seen to be affected. The hyaloid artery of the eye, present in the earlier and already absorbed in the later inter-uterine stages, is sometimes persistent after birth. Liebreich records, in the Transactions of the Pathological Society, an instance of the persistence of this artery in a boy of sixteen. In New York Dr. Callam found three cases of persistent hyaloid artery in examining the negro school children, and it is a question if this disability is not more frequent in the inferior races, as, indeed, we should expect it to be. There can be no design in an imperfect sense organ, but imperfect sense organs nevertheless exist. And, indeed, any study of the eye is incomplete without studying the eyes of lower animals. In studying the development of flounders, Prof. Alexander Agassiz found that at a certain stage of growth the right eye sunk into the tissues of the head, penetrating into the space between the base of the dorsal fin and the frontal bone. Prof. Agassiz has been able to follow in one and the same specimen the transition of the eye from one side of the head to the other. With this change in the position of the eye is associated a change in the position of the body when swimming, for before this the flounder swims vertically, but at this time it commences to lie on one side. We may conclude with Wilson, that since this distorted condition is shared in a lesser degree by a variety of other fishes, and thus a succession of steps is furnished, that it points to an adaptation by inheritance, although it is possible that there is a nearer solution to be drawn after we know the habits of the flat fishes more fully. But throughout the scale of animal life, the senses are seen to adapt themselves everywhere to their surroundings. The eye, which in man is moveable in its socket, and has a pupil which may be dilated to accommodate itself to the different rays of light, ceases to present these and other adaptive characters as we descend the series of animal life. We come at length to the special series of blind animals, such as the eyeless crab, *Willemoesia*, found by the

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Challenger expedition in the ocean depths, which wants even the eye-stalk and pedicel, and seems to have no occasion for sight where the light will no longer penetrate.

To the conception of design is linked the idea of creation out of nothing. Both arise from the limitation and imperfection of our sense organs, and my thesis is, that a belief in creation and design as opposed to evolution and adaptation is the result of being satisfied with the surface impressions we receive, and not checking and testing the results we obtain from one source of knowledge alone. There are certain Indians in the West, of whom Major Powell tells us, who believe that grass grows, but that trees are created. And among them are likewise certain philosophers who think that both grass and trees grow, and who have a hard time of it in consequence, and with whom some of us in more civilized society may sympathize, though at a distance. It must not be taken for granted that even the slight extension of the conception to include trees makes intelligent men out of these Indian philosophers; it shows that there are many grades of intelligence in this direction. The sources of intelligence are so various that we cannot come to understand the result, as displayed by the individual, without knowing more facts than we can get at. If we know the environment we can know the individual, and this fact is virtually admitted by all students of character. But to know the environment, how difficult! one is tempted to say, how impossible! But, perhaps, we can follow out our argument and at the same time attempt some classification of our knowledge, which will assist us in the study of the effect of the environment on the mind.

We have seen that we may conceive our knowledge as the total result of our sense impressions, and education as the process of acquiring these impressions. When we examine the sources of our knowledge, it will be found, I think, that we have attained it in two kinds of ways, and that for convenience and a better understanding of education itself, we may take these two ways as a rough classification of our knowledge itself. The one is by direct experimental contact with things, and this is the most positive and certain, so that we may call the knowledge we acquire in this way *real*. The other is through books and the teaching of persons other than ourselves, and we may call this kind of knowledge *second-hand*. In the process of education, we draw upon both these sources of information, and both go toward determining our

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total mental status. They are, in fact, so interlaced that we have difficulty in separating them. What we have read about often seems only to be separated from what we have experienced by its lesser vividness, and the art of teaching evidently lies in the power of presenting second-hand knowledge so that it has the force, or nearly so, of an absolute sense presentation. Undoubtedly there is a certain ease and facility in acquiring second-hand knowledge which renders the process attractive to the mind; the mind, which, as we have said, grows in the direction of receiving information. But this second-hand knowledge carries with it the greater possibility of error. We have only to recollect that our second-hand knowledge is imparted to us through the machinery of words, and words, we all feel, but approximately express our ideas. This fact supplies the reason for the success of object teaching in education. Undoubtedly the second-hand information of to-day is not the best information now possible, but, as a whole, it compares favorably with the second-hand information of twenty years ago. From the fact that the vast majority of us acquire our conceptions of external things largely during our school and college days, there is an evident reason for the popular unfriendly attitude towards new ideas drawn from experiments unknown to the past generation of school children. Between this second-hand information and real knowledge there is seen to be a constant interaction; the first is always improved by the results of the latter, and so mankind is instructed as time elapses. The proceedings of our scientific bodies contain a mass of information which is brought later on into our school-books in different shapes. The effect on the minds of the rising generation is cumulative, and most children start with ideas and a presentation of facts with which their parents perhaps finished their education, and few of us, we know, actively pursue our studies during mature life. But here we must be struck with the fact which a study of the inheritance of certain faculties of the mind presents. Much, we know, almost everything, is transmitted from the parent to the child, and along with the copy of the structure, the resemblance is carried out into minor details of form and feature. And in the same way the structure of the brain, which we yet fail to understand well, is affected. The faculty for receiving a certain class of brain pictures rather than another, the bent of the mind in a certain direction, follow with the color of the hair and eyes and the

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shape of the skull. But this faculty may exist, and at the same time an absence of the brain-picture, which would satisfy and fill it, may cause its obliteration in the child from disuse. We must then clearly distinguish the factor of heredity as affecting the sensory nerves and the brain, when we consider the total mental condition of the individual. The action of the senses, and the exercise of the brain beyond a certain extent, which varies with the individual, are painful and therefore distasteful. Up to what point they may be carried in any given case is difficult to determine. Sufficient it is for us to appreciate that, for the mass of mankind, a small total amount of sense and brain work suffices, and that we are generally willing to avail ourselves of the less tediously acquired knowledge which we receive through books. And even these it is almost usual to read carelessly and to avoid comparing, taking the remarks of author after author listlessly into our minds, until our reason is clogged with contradictory impressions and our total mental attitude becomes feeble and vacillatory. And here this fact presents itself, that in proportion as we apply to the sources of our real knowledge, and generalize from the results of our sense impressions, we are able to criticize our second-hand information and work toward a truer apprehension of ourselves and the world we live in. Those who rely chiefly upon second-hand knowledge in effect refuse the present opportunity, which is alone their own, and must necessarily accept a lower philosophy of their lives. And by philosophy we mean, after all, an explanation of ourselves and the world in which we are. The range of meaning in words is so great that ordinarily we conceal under this term one knows not how much that is mysterious and that may be even held unnecessary. But by philosophy we evidently mean that correlation between the brain-pictures by which that which is contradictory is explained or brought into its true succession. We all know how comparatively easy it is for us to entertain contradictory beliefs, and how we do not even notice this contradiction until we come to compare our ideas, which we do not all of us try to do. But something of a philosophy we all of us attain to as the natural result of our sense impressions. As we rise to general conceptions we bring this philosophy to bear upon them, and we are apt to answer very difficult, even unanswerable questions, in a way which at once measures our knowledge and tries our judgment. Even when our philosophy gets damaged by

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facts, we go about with it, and wear it in some fashion, still preferring the old cover to the exertion of getting a sounder one from new sense impressions and a sounder correlation of ideas. But our philosophy or mental position towards the rest of the world is of vital importance, since from it we derive much of our happiness. A life is often wearily spent in struggling to face even very ordinary mental difficulties, in the endeavor to reconcile our experiences with our derived ideas. What is needed, then, is some more accurate comprehension of what we are and to what things are tending, and to get at this we must lay under contribution all the possible sources of knowledge. From a conscientious application to the evidence of our senses, we may come to some certainty of what things are and have been, and from a careful study of literature we may find out the direction of development in human affairs. The result of both these lines of investigation to the student is a culture as high as the amount of labor he has expended, and a happiness as complete as his mental development is equal-sided. In any event, this culture will be found to confer upon its possessor immunity from many of the sorrows of this life.

But obviously this total happiness depends on the correctness of the philosophy upon which it rests. A picture on the brain, received how it may be, if partial only, is nevertheless lasting, and has its effect in deciding the mental tone. And from these incomplete pictures we arrive at false or incomplete conceptions. In this way the persistence of ideas may be explained by the persistence of the physical impress on the brain; and the mental labor and pain consequent requisite to alter or erase such a partial and misleading picture is always great. Great in any event, it becomes too heavy a task for many of us, when the pictures have most of them taken this turn. We are then committed to a wrong view of life, and must suffer the consequences. We must share in this event the average happiness which the defective philosophy, to which the pictures lead us, ensures. And the way in which pleasure and pain generally arise is the same as the way in which they come to the sense organs and the nerves. Pain arises in the body when any sensitive portion is torn or interfered with, and again where the wearing of the tissues is greater than the supply from the blood of the waste, as in being tired, or hungry, or thirsty. Pleasure arises from as vigorous an action of the organs

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of the body as can be maintained without expending more force than is supplied to the tissues and nerves from the blood. Exactly in the same way, by wear and tear, we find our senses affected; so that the conclusion is inevitable that the injury we inflict to the total organism, from undue exercise of the senses, is as pernicious to its proper development as excessive muscular work would be. The intellect, as distinguished from the emotions, is that part of the mind which discriminates and appreciates differences, but this fails us, too, after severe studies. A hard example in arithmetic, for instance, and a difficult feat in gymnastics, present similar demands on the system; and at a certain age, indeed, the amount of vital repair necessitated by the first may be greater than that demanded by the second.

To trace the growth of the human intellect, from before the earliest historic period until the present time, is a work of immense labor, and is one that is engaging the attention of the greatest minds. I can only attempt in a simple way to give here the more evident marks of its progress, and which I have on another occasion brought briefly before this Association.

But before doing so, we may refer to our present knowledge with regard to fossil man. In a late address, Dr. Virchow has seen fit to dispute the value of this evidence as bearing upon man's evolution from a lower type of animal life. In effect, the evidence being fragmentary, is also to some extent contradictory. But leaving out of sight the proofs from American sources, brought together by Prof. Morse in his address before this Association, Mr. Henry Gilman's researches in Michigan, and the subsequent discoveries of Dr. Abbott in the gravels of New Jersey, I wish to point out that the capacity of the skull is no absolute criterion of the intelligence between certain limits of measurement. As a fact, the heaviest brains yet known did not belong to persons of the greatest mental power. Whatever, then, the capacity of the prehistoric skulls of Europe, the implements which their owners fashioned, the relics which we possess of their social life, are proofs of the low intelligence of these ancient men. Whatever their brains might have weighed, the direct evidence is that these people were far below the races that have succeeded them in knowledge. The opportunities of the cave-dwellers were not golden, but flinty; their experiences few, their lives poor. Unless we are willing to cancel all we have yet gathered from

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antiquarian sources, we must admit that the evidence, as it stands, makes directly for the argument of mental evolution. And looking at this change in the minds of men through the historic periods, and which is going on to-day, we may trace it to material causes, because we must consider that education and intelligence have a material basis, as I have tried to show in this address. And we cannot separate, in any essential way, the development of the mind and that of the body, the one being conditioned by the other. That development seizes on certain organs or parts of the body especially, we know from the studies of Profs. Marsh and Cope among our own palæontologists, and it is quite clear that in man the development of the body has for some time been less active than the development of the brain. It is the immediate environment which acts on the organism; and man, in social communities, is no longer chiefly dependent upon muscular vigor, but upon mental activity, for a successful existence. But corroborative evidence of the development of man from a lower type of animal life, we find in the fact that for every bone, muscle, or organ in man, there is a corresponding one in the anthropoid apes. It must not be left out of sight that any special distinctions of this kind between man and certain apes have been found totally wanting. All the differences now recognized, including that of the cubical contents of the brain case, have been found to possess only a comparative value from their range of variation in different apes and in different men. This much we can, therefore, clearly show, that the difference between man and certain lower animals is a quantitative, not a qualitative one. With this, and the arguments from embryology and from mental evolution, the case of the total development of man stands, we must think, on very firm ground, even without the assistance of the analogies to be made with the development of other organisms, and without drawing on the possible future discoveries of palæontologists, the character of which might be justly inferred from what has been already presented.

A work of immense labor it is to unravel the network of complex thought which surrounds us to-day, and determine the origin of the separate threads. Such a labor is comparable to that of the biologist, who, through a succession of different but allied species of animals, traces the origin and modifications of a bone or muscle. But so much exists upon this subject already that I have ventured to give the material points of our mental progress, which may

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possibly assist us in our conceptions of the successive stages through which the mind of man has already passed. At the outset, we cannot too strongly insist upon the comparative value of such terms as "civilization." Our general tendency is to give a fixed value to such words, measuring them by our own standard; and to this unconscious action, which has a wide application, and from which nothing but culture will free us, deeper thinkers have given the name anthropomorphic. In this instance, since our civilization is constantly changing, we can only consider ourselves as standing on a plane of comparative excellence, while a comparison of our own with the civilization of the Chinese, Hindoos and Japanese, shows us that in some few points, it may be, we have a lesson to learn; while in others, as in the character of criminal punishments, these nationalities occupy ground that we have abandoned centuries ago. The motor of civilization must always consist in an improvement of the machinery for the exchange of thought. This follows from our conception that our intelligence results from our sense impressions on the brain, and that we understand by education the process of acquiring these brain-pictures. Clearly, the acquirement of articulate speech marked the first advance in human communities in mental evolution. It does not appear how articulate speech came to us, although we may hope that in the disintegration of vocal sounds, by some such invention as the phonograph, a reasonable theory of its successive steps may be brought out. The first marked advance upon the formation of language lay in the discovery of the characters of writing which we call phonetic, the steps to which were marked by the use of different grades of object signs. Through the invention of writing, a man's experience in this world was not lost when death struck him dumb. For the earliest writings we have retained a special reverence which lasts until to-day, and has not been dissipated by extended familiarity with the process. The next step lay in the invention of printing — always following the same line of advance which mental development had marked out from the first, the growth of the mind toward receiving more and more perfect brain-pictures of external things, and the effort to assist this growth through the dissemination of ideas. Before the invention of printing, an important part of the process of education was limited to the few, because these alone possessed the art of writing and the means to secure collections of manuscript of different kinds. Hence the difference

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between ancient and modern civilization lies chiefly in this, that in the former the masses could chiefly be improved from outside of themselves by the orator. The printing press, at a later time, enabled the people to take the orator home with them, and review at any time his messages. In ancient times writing required to be read, and the stated re-reading came to be a custom in public, before the action graced the hearth at home. So we may consider the advance of mental intelligence to be marked by these three great inventions: speech, writing and printing, while in our own day the spreading and perfection of our intelligence have been aided by the kindred inventions of the telegraph and telephone. Civilization thus appears as the consequence of the dissemination of experiences among mankind.

Those who have brought together the story of the ancient civilization of Greece have agreed with unanimity that the separation between the mass of the people and the intellectual portion became at length insurmountable, and finally led to national destruction. This makes for our own view, that it was to a defect or incompleteness in the machinery for the dissemination of knowledge, that we must ascribe the dying out of the older states. An intellectual aristocracy was established in Greece, which, in order to maintain its superior position, and thus, from natural and selfish motives, endeavored to prevent the spreading of new facts, but it was assisted in this action by the limitations which an ignorance of the art of mechanically duplicating writing, threw around it. Philosophers have explained the fall of Greece, by considering it as a necessary step in the progress of humanity and the perfection of a future bloom of knowledge. And so in one sense, it may be; but still exactly where the defect lay, and where there is a positive advantage in the conditions of modern civilization, and wherein modern civilization more adequately protects the state, have sometimes escaped them. To understand this fully we must come back to natural history, to anthropology, at last. A large class of persons with a certain bias persistently decry our modern civilization, and look for its more or less speedy evanishment, merely because Rome perished and Greece decayed. But nowhere in nature is there exact repetition, and to understand the new civilization we must remember that it rests on a larger average intelligence, brought directly about by the discovery of the art of printing. There is then a distinct reason, a scientific

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ground, for the opinion that our present civilization rests upon a surer basis than did those which preceded it, and this we may safely bring forward in the cause of truth. For science is in danger always of being regarded as the enemy of the state, because it tends constantly to modify existing ideas. But if we can show the necessity for a constant modification of our ideas, arising out of our own constitution, then it may be seen to be unreasonable to defame those who follow the search for truth. And it being undoubtedly true, as Locke says, that of all the men we meet with, nine out of ten are what they are, good or evil, useful or not, by their education, we can see how wide-reaching the effect of our improved basis of civilization must be upon us as a people, and how important it is to understand the real direction in which it works.

But, indeed, the position I have tried to sustain in this address, lies outside of any criticism of modern education. I have tried simply to show the way in which our modern civilization has grown up, and its real superiority over ancient culture. From this we may rest assured that science, while it influences, can never be an enemy of the state, and that the danger of the state, as well as other social systems within the state, will lie in the direction of an opposition to scientific truth and the right reason of mankind. But it remains for science to play a distinct part in the discharge of its full duty to the community, by popularizing its discoveries. Doing this, it will insure the stability of the state by increasing the general information of the people. Through the instrumentality of publications, particularly such as the New York *Tribune Extras*, the *Popular Science Monthly*, and the *American Naturalist*, this work of reaching the people is being specially undertaken. But indeed the work of science is being aided generally by the newspaper. And in this work an association such as that we enjoy can take an active part by giving popular lectures. There rests upon us, then, a responsibility deeper than the perfection of our own knowledge —we have to spread it in the interest of the whole state. Gratitude to those who generously entertain us at our annual meetings should, at least, prompt us to give in return the knowledge we possess in its easiest shape, while we may confidently hope that in so doing we are aiding the prosperity and peace of our common country. And beyond these there is the future Man for whom we are always working, and who is to be wiser and happier than we can ever be.



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The demand has come up from teachers throughout the country that they should be better informed as to the manner in which the sciences may be introduced into the schools and the matter to be taught. It is the duty of this Association to furnish the information. If we have not sympathized with this inquiry in the past, let us assist it in the future. It is quite evident that the sooner this Association commits itself as a matter of principle to the furtherance of science among the people, the more following it will have and the greater influence. And if it does not it will fall behind its peculiar duty and out of the line of advance in human thought. This Association must be prepared to demand more time for scientific studies from the public school authorities, and it must show to every one that education is a matter which not only falls properly under its cognizance, but which it is also prepared to take hold of. This Association should no longer delay to bring all its forces to bear upon the question of science as applied to education. While it does not do so, it will always seem to shirk a duty and ignore one chief end of its existence.

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# PAPERS READ.

## The Relation of Adhesion to Horizontal Pressure in Mountain Dynamics. By H. F. Walling, of Cambridge, Mass.

Geologists of to-day are quite generally agreed in attributing to horizontal pressure much of the folded and otherwise disturbed condition of the rocky strata which is observed in and near mountain chains. Rocks are found tilted and folded in such a manner that they occupy less horizontal space than when originally deposited in a nearly level position. The conclusion is obvious that the force which effected the change, if not a horizontal one, must have had a large horizontal component.

Cataclysmic hypotheses, according to which the earth's crust has been suddenly rent asunder, folded up into ridges and otherwise distorted by the pent-up, heat-generated forces below, or thrown into waves by the fall into the molten interior of a crust too large for the contracting nucleus, etc., although formerly entertained by such eminent men as Humboldt, Agassiz, Ritter, Von Buch, Beaumont, Werner, Rogers and other noted geologists, have been almost entirely abandoned for hypotheses less discordant with the gentle and gradual processes which, on a more complete study, one finds indicated in the appearances of the rocks.

What is called the "contractional theory" was first proposed by Professor Sedgwick in 1831,[1] and has since been quite generally accepted by geologists. According to it the folds of the earth's crust are due to its adaptation to a contracted interior, and the comparison is sometimes made between the folds which are supposed to constitute mountain chains and the wrinkles in the skin of a shrivelled apple. Recent examinations of this theory, however, seem to prove its entire inadequacy to explain observed phenomena.

Capt. C. E. Dutton, of the U. S. Geological Survey under Major

[1] "On the Geological Strata of the Cambrian Mountains." Trans. Geol. Soc., Jan. 5, 1831. In a recent paper by Professor Alph. Favre, see note, p. 10, he remarks that Descartes as early as 1644 had suggested a similar hypothesis to account for the "dislocation of rocks," Œuvres de Descartes, Paris, 1824, III, p. 366, § 42.



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Powell,[2] after a careful analytical discussion of the problem, based upon the mathematical investigations of the laws of the "Conduction of Heat," by Fourier, and of the "Secular Cooling of the Earth," by Sir Wm. Thomson, assigns, for the limits of the sensible cooling of the earth, horizons not more than two or three hundred miles below the earth's surface, and says[3] that "although no estimate can be made of the contraction of this portion" (above the limiting horizon) "it is probably safe to say that its volume cannot have diminished so much as one-tenth; and if we were to assign thirty miles as the diminution of the earth's mean radius since the first formation of a cooled exterior, we should probably reach the utmost limit consistent with Fourier's theorem. By far the larger portion of this contraction must have taken place before the commencement of the Paleozoic age. By far the larger portion of the residue must have occurred before the beginning of the Tertiary; and yet the whole of this contraction would not be sufficient to account for the disturbances which have occurred since the close of the Cretaceous. In all mountain regions the disturbances of the strata, which are supposed to be due to tangential compression, are so great that in order to account for their plication by this hypothesis we should be compelled to assume a contraction of some circles of latitude, since the commencement of the Permian epoch, amounting to many hundreds of miles. But when we examine the Laurentian rocks wherever found, their excessively disturbed condition must utterly prohibit the belief that it is the result of secular contraction of the interior. Bearing in mind that a shrinkage of one-fifth of linear dimensions implies an increase of ninety-five per cent. in mean density, and that according to this hypothesis such increase is zero at the surface, it puzzles the imagination to conceive what must have been the condition of the earth mass while the Laurentian sediments were accumulating, if we are to assume that their present distortion is due merely to secular contraction."

Rev. O. Fisher (Trans. Camb. Phil. Soc., vol. xii, p. 414), upon an elaborate computation, using Sir Wm. Thomson's results, finds that in order to produce a horizontal compression in the solid crust, such as geological sections indicate in regions far from

[2] On the Contractional Hypothesis. Am. Jour. Sci., 3d ser., VIII, 1874, p. 113. See also Penn. Monthly, May, 1876.

[3] Am. Jour. Sci., VIII, p. 121.

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being violently disturbed, a diminution of the earth's radius of from 300 to 600 miles must have taken place, which would give a co-efficient of expansion far in excess of that obtained by Mallet's experiments upon melted slag.

"Nevertheless," he says (p. 430), "from appearances I think we must be prepared to give up our contraction theory altogether or to admit a change of dimensions approaching to such an amount." He suggests, however, that the earth may have been cooled from without inward and that the water of the ocean may have been in great part held in a sort of aqueo igneous fusion escaping when the rock became sufficiently cooled.

Capt. Dutton also shows very clearly the mechanical difficulties in the way of folding rocks in the manner seen on the earth by a tangential pressure derived from the contraction of the supporting interior mass. This involves a large amount of *slip* over those areas which are not corrugated. The adhesive force between the crust and the nucleus, even though the latter were covered by liquid lava, would be something enormous under the pressure of thousands or millions of cubic miles of rock spread upon the vast areas where the strata show no signs of disturbance, as, for example, throughout the Mississippi valley. Moreover, if the earth is still hot enough for a non-viscous liquid layer of melted rock to exist between the crust and the solid nucleus, the amount of cooling and contraction must have been far smaller than that already supposed, and therefore still more in deficiency for balancing the diminution of width in ground plan occasioned by plication.

But the most eminent geologists and physicists find strong physical reasons for believing that the condition of the material immediately below the solid crust, although probably hot enough to be liquid if relieved from the pressure of the rock above, is in reality that of a plastic semi-rigid mass, perhaps resembling that of iron at a temperature somewhat below that of its fusing point.[4]

[4] Professor J. D. Whitney (Whitney on Earthquakes, Volcanoes and Mountain Building, p. 75), while admitting the probable solidity of the earth's interior, says "The late researches in physics, however, have shown that there is no such sharp line dividing solids from liquids as was formerly supposed, and that all the requirements of geology would be satisfied if it should be admitted that the material constituting the earth's interior, if not already in the fluid condition, was capable of assuming it when relieved of pressure to a certain extent."

Rev. O. Fisher, (Trans. Camb. Phil. Soc., Vol. XII, p. 433), taking into account the small specific gravity of the earth, concludes that a "superheated condition exists at no very great depth below the surface. By which I mean that if it be solid the solidity is due to pressure."

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If this be the real condition of the supporting mass, it must oppose an enormous resistance to the sliding of the crust upon it, which would effectually prevent the accumulation of the corrugating effect in narrow belts of supposed weakness.

It is the object of this paper to show that the adhesion, which would thus resist a contractionally derived force, affords a means of efficient application of the real force to which rock-folding is to be attributed.

Capt. Dutton also shows that the force which would be developed by terrestrial contraction would act in all directions alike upon a spherical surface. The collapse, if it occurred, must have been equal along every great circle. These necessary consequences are fatal to such a hypothesis in regard to existing mountains. Moreover, the attempt to raise a broad flat arch of one or several miles' span by horizontal pressure against the ends, as any engineer would predict, would be futile. No rock, or any other known material, could withstand the tremendous pressure required to produce such an effect. The elevation of great continental arches in this manner is evidently still farther beyond the limits of possibility. Rev. O. Fisher[5] has shown that the crust of the earth, if unsupported from below, would sustain on any vertical area a pressure equal to the weight of a column of rock of the same density and area of cross section, having a height equal to half of the earth's radius, say 2000 miles (3200 kilometres), or more than 10,000,000 pounds to the square inch (700,000 kilograms to the square centimetre)!

Capt. Dutton's conclusions are controverted, however, by Prof. Joseph LeConte, who comes to the rescue of the contractional theory in a recent and very instructive paper "On the Structure and Origin of Mountains."[6] In this he insists upon considering the agency of terrestrial contraction in producing folds in rocky strata as a fact established by observation, a "Formal Theory" which "discusses and determines the laws of phenomena and the conditions under which they occur," in distinction from a "Physical Theory" which "discusses the physical cause of these laws" (p. 107).

Is not this a begging of the question? This, I take it, is not whether horizontal pressure has been exerted — that is evident, —

[5] Trans. Camb. Phil. Soc., Vol. XI, p. 489.
[6] Am. Jour. of Sci., Aug., 1878, Vol. XVI, p. 95.

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but whether the contraction of the earth is the only or the principal cause of this horizontal pressure.[7]

In looking for its true cause, let us first accept the principle so well taught by LeConte in regard to the origin of continents, and which he says "had been previously brought out by Professor Dana and Archdeacon Pratt," (he might have added that Sir John Herschel proposed it more than forty years ago), attributing "the *greatest* irregularities, constituting continental surfaces and ocean bottoms to *unequal radial contraction* or a *secular deformation* of a heterogeneous earth;" that is, the figure of the earth is considered to be one of gravitational equilibrium. In fact, Archdeacon Pratt has found evidence going far towards a mathematical proof of this hypothesis, in the careful study which he made of the figure of the earth from data in regard to the direction of the plumb line in different places obtained in the great geodetic surveys of India and other parts of the world, and from pendulum experiments for determining variations in the force of gravity, performed upon high plateaus, as well as upon levels but little above that of the sea. He says[8] "The variety we see in the elevation and depression of the earth's surface, in mountain and plain and ocean beds, has arisen from the mass having contracted unequally, in becoming solid from a fluid state, and that below the sea level, under moun-

[7] Professor Benjamin Peirce, (Proc. Am. Acad. of Arts and Sciences, Vol. VIII). after showing that for a two per cent. lineal shrinkage, a probable cooling of the *whole earth* of not less than 2000° Cent. must have taken place, "which would require that its original temperature should be higher than would be consistent with the solidity of these shrunken strata," suggests that "another source of change of form, which would produce shrinkage in different directions in different parts of the earth, is to be found in the diminution of oblateness arising from the diminished velocity of rotation upon the axis." After remarking that the lunar action upon the tides is the only known cause for reducing the earth's velocity of rotation, he shows that this reduction could not have been from a greater velocity than 4,236 times the present, which, to maintain equilibrium of form, would make the equatorial axis two and one-half per cent. greater than at present.

But if the above-mentioned geological considerations are admissible, it becomes evident that even adding the amount of deformation possible from diminished oblateness to the contraction caused by cooling, and localizing their united effects in the disturbed belts, the sum would still be wholly inadequate to account for the amount of corrugation observed.

It will be observed that diminution of oblateness is not contraction in the sense of increase of density, but rather deformation. It requires a *movement* of some part of the earth's mass from equatorial to polar regions. According to the considerations hereinafter presented, the direction of the axes of corrugation should be normal to that of the movement, and the principal effect of plication should be in regions intermediate between the equator and poles. While there would be a diminution of ground plan in the direction of rotation, there would be an extension in meridional direction throughout equatorial regions, so that corrugation could hardly be expected there.

[8] Phil. Mag. XLI, 1871, p. 307.

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tains and plains, there is a deficiency of matter approximately equal in amount to the mass above sea level and that below ocean beds there is an excess of matter approximately equal to the deficiency in the ocean when compared with rock; so that the amount of matter in any vertical column, drawn from the surface to a level surface below the crust, is now and ever has been approximately the same in every part of the earth."

Sir George Airy, the present astronomer royal of England, confirms this view, and, while urging that the upper strata of the earth maintains a hydrostatic equilibrium, as though floating upon a fluid, remarks[9] "This fluidity may be very imperfect; it may be mere viscidity; it may even be little more than that degree of yielding which (as is well known to miners) shows itself by changes in the floors of subterranean chambers at a great depth, when their width exceeds twenty or thirty feet; and this yielding may be sufficient for my present explanation." He then goes on to show that certain discrepancies between theory and fact, arising in Pratt's computations, might be removed by supposing that the floating masses forming the higher plateaus are probably thicker and so sink deeper into the heavier material below, than those forming lower plains and ocean beds, in the same way that the larger logs of a raft would sink deeper into the water and float higher above it.

Archdeacon Pratt remarks that the configuration of oceans and continents indicates in a general way the equilibrium of heterogeneous materials. He says,[10] "This theory, that the wide ocean has been collected on parts of the earth where hollows have been made by the contraction and therefore increased density of the crust below, is well illustrated by the existence of a whole hemisphere of water, of which New Zealand is the pole, in stable equilibrium. Were the crust beneath only of the same density as that beneath the surrounding continents, the water would be drawn off by attraction, and not allowed to stand in the undisturbed position it now occupies."

If, in accordance with the above stated views, we admit that the earth has a rigid nucleus, surrounded by a crust, flexible like all known solid bodies under long continued pressures of sufficient magnitude, and that between the crust and the nucleus there is a

[9] Trans. Roy. Soc., CXLV, p. 101.
[10] Figure of the Earth, 3d Edition, Art. 138, p. 135.

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viscous or even nearly solid layer, yielding, perhaps very slowly, to unequal pressures, and flowing in the direction of that which preponderates, it seems to me that we have an adequate cause for strata-corrugation in the disturbances of equilibrium caused by denudation.

It is evident that if there were not some mode of restoration, the original equilibrium of gravitational pressures must have been greatly disturbed by the removal of materials from continents to ocean beds, which has gone on through long geological periods. But proofs are abundant, that while in many parts of the world sediments were deposited many thousands of feet in thickness, they maintained a nearly constant surface level, a little below that of the sea, showing that the crust beneath them was sufficiently flexible to permit it to sink into the yielding mass below as fast as weight was added above.[11] Examples of this kind of action are so numerous as to indicate a general law of maintenance of level, this level being subject, however, to secular changes arising from "unequal radial contraction" of the heterogeneous and slowly cooling materials, sufficient perhaps to account for past alternations between ocean and continent, as well as for the slow vertical movements now going on in different parts of the world.

Rev. O. Fisher, in a very interesting essay upon "The Inequality of the Earth's Surface, as produced by Lateral Pressure, on the Hypothesis of a Liquid Substratum,"[12] expresses the opinion that diversity of materials cannot explain the oscillations of continents and oceans, "For that would require the materials to become changed in their properties from time to time, in a manner highly improbable."

Is not the supposed change of properties here overrated? Is it necessary, indeed, to suppose any change of properties, if we assume, what is quite in accordance with physical investigations, that the rate of contraction or expansion, while passing through the same ranges of temperature varies in different substances, and is not in simple ratio to the rate of cooling in either? Since continuous cooling is inevitable, may not a more rapid rate of contraction of the substances under former continents, than that of the materials under oceans, have, at length brought about a sub-

[11] J. D. Dana, Am. Jour. of Sci 2d ser., XLII, p. 207. J. W. Dawson, Jour. Geol. Soc. of London, May, 1866, p. 95. James Hall, Paleontology of New York, Vol. III, p. 69.

[12] See Trans. Camb. Phil. Soc., Vol. XII, p. 434, Feb., 1875.

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mergence of the land and an emergence of portions of the sea-bottoms? There is no evidence that the *deep* ocean beds have ever been dry land, and the oscillations in sedimentary formations may not, in general, have exceeded two or three thousand feet, say twice the average height of continental plateaus. This is a very small percentage of the depth supposed to have been subjected, since solidification took place, to appreciable cooling, a depth variously estimated from eighty to five hundred miles.

Then it must be remembered that denudation brings about important changes in the *relative horizontal positions* of the earth's materials, both above and below the flexible crust. These changes would tend to gradually reduce inequalities of level, by interchange of material over different areas. Moreover, under denuded areas, matter from below is gradually brought nearer the surface and cooled, while the deposits are carried down into warmer regions and reheated; a sort of *solid convection* and agency of heat-dissipation, by the way, which seems not to have occupied the attention of those who have investigated the cooling of the earth's interior.

The maximum aggregate depth of sediments is, by recent estimation, 177,200 feet,[13] or about thirty-three and one-half miles. It is very probable that the areas of deposit exceed those of the denudation from which the deposits are derived, owing to the distributing action of the water in which the sediments are suspended. If so, the height of the areas cut down must have been proportionally in excess of the thirty-three and one-half miles of deposits. But taking them equal and supposing no change of surface level, that is, supposing the deposits to have sunk and the denuded areas to have risen, we have sixty-seven miles of relative change of level, which the denudation has effected between portions of the materials. This corresponds, at the present rate of downward increase of temperature, say $\frac{1}{5}\frac{1}{1}^{\circ}$ Fahr. per foot of descent at the surface, as taken by Sir Wm. Thomson, to about 6500° Fahr. of maximum relative change in temperature, allowing for the diminution in the rate of downward increase, with increased depth. That is, the lowest sediments would be heated 3250° and the matter brought to the surface from the lowest depths in denuded regions would be cooled 3250°.

[13] Notes on Physical Geology, by Rev. Saml. Haughton, read at Roy. Soc., April 4, 1878. Printed in Nature, Vol. XVIII, p. 266.

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May we not, accordingly, attribute to *denudation* an effective agency in aid of the production of secular oscillations in surface level, through the want of uniformity of expansion and contraction in redistributed heterogeneous materials?

Returning to the cause of folding, if the viscous layer acted as a slowly moving liquid, it would, according to the laws of hydrostatic pressure, while yielding to the excess of loading in one place, push up the crust in another, where weight had been removed from above by denudation. To maintain equilibrium, this action must be accompanied by a *horizontal flow* of the viscous matter between the two regions. This horizontal flow would be greatest in that part of its path between the sinking and the rising areas, where no change of level occurred, since a quantity of material must pass that line or belt equal to the whole amount displaced by the sinking crust.

Would not the *adhesion* between the crust and the stiffly viscous moving matter beneath it, be sufficient to cause the latter to impart its motion to the former, and since the rate of this motion would be different in different places, would not crumpling or folding of the strata be a necessary consequence? If so, should we not find the greatest amount of folding between the intermediate region of greatest motion above mentioned and the denuded plateaus? Or should we not find the principal mountain chains of the world parallel with, and not far from ocean shores, and that the sharpest and most numerous foldings would be upon the flanks of mountain slopes and parallel with the ranges?

An instructive experiment, illustrating this supposed mode of crumpling strata, may be performed with a strip of India rubber of convenient dimensions, say thirty or thirty-five centimetres long, two or three centimetres wide, and six to ten millimetres thick. It should be stretched sufficiently to add, say one-half to its original length, and, while extended, covered with a layer say seven or eight millimetres thick, of soft modelling wax. Upon this may be placed sheets of differently colored and somewhat harder wax, separated from the softer wax and from each other by strips of cambric. A slight degree of warmth and pressure will cause the different layers to adhere very strongly. On allowing the India rubber to shrink gradually to its original length, the effect produced upon the colored sheets of wax is curious and instructive. I found it convenient to prepare the colored sheets

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by melting white wax with a little venice turpentine, adding pigments in powder to give the desired colors. I made use of a shallow trough of tin-ware of suitable dimensions, which I slightly greased. A piece of cambric was then laid in it, and the melted wax poured upon it. The cambric thus used is sufficient to separate the sheets and to prevent lateral extension. On trimming the ragged edges with a sharp knife, after the India rubber has resumed its position and the wax allowed to become quite cold, a very pretty model of a geological section is seen, in which, however, portions denuded in nature are ideally restored. Of course the conditions are different in some important respects from those they are intended to represent, and the experiment only serves to illustrate the *folding of strata by adhesion*, during unequal motion of the supporting mass.[14]

[14] Since the presentation of this paper I have seen in the "Archives des Sciences Physiques et Naturelles de la Bibliotheque Universelle," Vol. LXII, Geneva, June, 1878, p. 193, an account of "Expériences sur les Effets des Refoulements ou Écrasements Lateraux en Géologie," par Professeur Alph. Favre. The experiments are very similar to the one above described, which I performed in May, 1878, quite in ignorance that any other person had made or was making similar experiments. I exhibited the wax model which I made in this way, to the Appalachian Mountain Club in Boston, June 12, 1878.

Professor Favre's experiments were performed with layers of clay instead of wax, but otherwise the similarity between them and my experiment is very great. In some of the experiments he augmented the adhesion between the clay and India rubber by nails projecting into the latter and imbedded in the clay. The description of these experiments is beautifully illustrated by nine lithographed figures of the clay bands as they appeared after the compression and corrugation. Many remarkable resemblances to actual mountain formations are designated in the descriptions.

When the experiment was performed *without adhesion*, that is, upon a smooth oiled board, by compression alone, the clay was only slightly wrinkled in the middle parts of its length, but the effect was to form bunches of clay at the ends, against the blocks of wood through which the longitudinal compression was effected, by causing them to approach each other. It will be seen that while my experiment was made for the purpose of testing the effect of cohesion in producing corrugation, Professor Favre's were made to test the operation of lateral pressure, but were unsuccessful until cohesion was brought into play.

Professor Favre also attempted to imitate the supposed pressure upon the strata of the earth *in all horizontal directions*, by operating with a sheet of India rubber, sixty-six centimetres in diameter, stretched so as to form part of the surface of a sphere with a radius of forty-one centimetres. He placed layers of clay upon the stretched convex surface, and allowed the India rubber, little by little, to shrink to a plane surface. But although repeated trials were made with layers of clay varying in thickness from two to thirty-five millimetres, no satisfactory result of corrugation was obtained.

What is to be learned from these experiments? If they prove anything, is it not that when adhesion to the slowly moving material below is sufficient to overcome the rigidity of the upper strata, corrugation is effected, *but not otherwise*? It seems to me that if this be admitted, the theory of a world-wide force of compression, arising from internal contraction, and "determined into narrow belts of weakness" through absence of adhesion between a rigid crust and a liquid layer beneath undisturbed regions, is utterly overthrown; for this involves the same absence of adhesion under the disturbed regions. Hence the corrugation in the narrow belt must be the effect of simple pressure

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If the rocky strata of the earth are compressed in one or more parts of a great circle, they must evidently, to compensate, be extended nearly the same amount in other parts, the difference being the amount of actual secular contraction from cooling. May not joints, dikes, etc., be the effects of such extension? In the case of joints may not the deficiency of breadth be distributed, in minute cracks, over areas of widely diffused sediments, by the gradually variable rate of flow in the viscous beds to which the strata adhere? If the action were more rapid might not the stretching be sufficient to open fissures extending from the surface down to the regions where the heated matter is held in a rigid or viscous condition only by pressure, so that it would liquefy, and rise in the fissure on being relieved from pressure? In another paper I have described some supposed examples of this kind of action in the trap ranges of the New England Triassic formation. (See p. 191.)

The general conclusions arrived at in the present essay may be summed up as follows:—

1. The great inequalities of the earth's surface, its continents, high plateaus, and ocean beds, are due to the heterogeneity of its upper materials, the lighter floating higher than the heavier upon a viscous or yielding substratum.

2. Mountains are *maintained*, not *elevated*, excepting that where erosive sculpture has produced great irregularities of surface, the peaks and ridges are carried up sufficiently to maintain the average level required for equilibrium.

3. Folding, or corrugation of strata, is produced through the *adhesion* of the upper strata to the viscous layer beneath, which in its movements to restore the equilibrium, disturbed by the transference of materials which has been effected by denudation, reduces the horizontal width of the supporting viscous layer, and, with it, that of the supported strata above. The folding is not caused to any great extent by lateral pushes against the two

against the two opposite sides, and this is shown by Professor Favre's experiments, as well as by the mechanical reasoning of Capt. Dutton, to be impossible.

As might have been expected, movements uniformly distributed in all directions, such as are produced upon its surface by the contraction of a sphere, did not in Professor Favre's experiments, even when transmitted to the overlying clay by adhesion, produce ridges, etc., representing mountain chains.

Are we not justified in concluding that the horizontal components of the forces, which really produce strata folding, are normal to the axes of denudation and are transmitted from the moving mass below by adhesion?

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opposite sides of the folded region, but by a force producing a diminution of ground plan, *distributed entirely across the folded region, and applied to the rocks above, through their adhesion to the supporting mass.*

4. The regions of greatest relative or differential underground movement are the regions of greatest surface disturbance. This disturbance under ocean bottoms is generally one of *extension*, but widely distributed, and usually manifested in numerous minute cracks or joints, except in special cases where fissures and dikes are produced by more rapid stretching. On denuded slopes the disturbance is from a force of *compression*, causing folds, etc. The most favorable locations for powerful action of this kind are the slopes of continental plateaus, as, for example, the Himalaya plateaus and those along the western coast of America.

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SOME INDICATIONS OF RECENT SENSITIVENESS TO UNEQUAL PRESSURES IN THE EARTH'S CRUST. By H. F. WALLING, of Cambridge, Mass.

*New England Triassic Region.* Supposing that the Triassic formation in the Connecticut valley would be more likely than the other more disturbed regions of New England to retain indications of yielding to unequal downward pressures of irregularly denuded upper strata, I recently passed a few days in examining the dips of the strata in the northern part of this formation, more particularly in the neighborhood of Mount Toby, in the towns of Leverett and Sunderland, Franklin county, Mass.

*Mount Toby.* Mount Toby, or Mettawampe, as it was named by Dr. Edward Hitchcock, rises to a height of about 1200 feet above the level of the sea, or 1000 above the level of the Connecticut valley in its neighborhood, and forms a ridge of this height about a mile and a half in length, having a direction a little east of north, with several irregular spurs, two or three hundred feet lower along their summit. Its base covers an irregular oval measuring some four miles from north to south and two and a half

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from east to west. It extends quite to the boundary of the Triassic on the east, is within about sixteen miles of the most northerly outcrop, and four miles of the western side of the formation.

*Dip of the Region.* The general dip of the formation in the Connecticut valley is towards the east, and there are no sharp folds, faults, or other indications of disturbance, than a general relative uplifting of the western side of the basin. Professor Edward Hitchcock, who made the only authorized survey of this region in Massachusetts, came to the conclusion that the strata were deposited in a horizontal position and have since been tilted up on the west side with the included trap dikes and beds. He sees no reason to suppose that the trap out-flows have been instrumental in the uplift.[1]

*Trap Rocks.* Dr. Hitchcock found the trap interposed in successive beds between the sandstone. He found no visible dikes in Massachusetts, though he supposed that they existed under the beds and that "through them the trap ranges were protruded."

*Mode of Formation.* The sandstones and conglomerates of this region are made up from the detritus of the older rocks around and were, doubtless, the result of the denudation of the granite hills on the east, west and probably on the north. If the "*adhesion theory*" proposed by me in another communication be admitted, we may suppose (see p. 189), that while the basin was being loaded down with deposits from both sides, movements of the plastic material underneath took place towards the east, the west, and in some localities towards the north. This would produce a *stretching* of the crust in these directions owing to its adhesion to the viscous matter below.

Might not this stretching have produced, in some places, vertical openings of sufficient width and depth to permit the highly heated material below, which would thus be relieved from pressure and rendered more fluid, to rise in the crevices so formed and even to overflow the surface, under such conditions that the locality overflowed would be below the level of the earth's surface, supposing its crust were all melted and homogeneous with that underlying the region considered, that is, below the level surface for matter of that density?[2]

[1] Report on the Ichnology of New England, pp. 14–17.

[2] The specific gravity of several specimens of trap which I gathered in this region

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*Variations in the Dip.* While the general directions of the dips are easterly, averaging about 50°, there are some exceptions, and these exceptions, so far as they have come under my observation, or are described in Dr. Hitchcock's Report, seem to me to be in accordance with the equilibrium theory, and to indicate a sensitiveness of the earth's crust to unequal pressures. The exceptions, says Dr. Hitchcock, are found chiefly near the margin of the sandstone. "Thus on the Springfield section it will be seen that there is a westerly dip as we approach the hypozoic rocks in the west part of Westfield. Again on the Turner's Falls Section, near the east end, we find somewhat of an anticlinal axis, giving a slight northwesterly dip, though the stratification at this point is obscure. In Hatfield also I have observed a westerly dip, extending even across the river to the granite of Mount Warner, as shown in the Norwottuck section. The strike in general is northeasterly and southwesterly, ranging, however, between north and south and east and west in particular places. *This is particularly the case where the trap ranges curve so as to run in the same direction, as at Mount Holyoke and in Gill.*"[3] Dr. Hitchcock also remarks (see Report, p. 11) that "the lower beds have upon the whole a higher, though not much higher, inclination than those above the trap. It is not so obvious on the Turner's Falls section, as it is upon that crossing Mettawampe, which mountain shows an average dip of only 5° to 10°. *The same thing is obvious by comparing the strata west of Mount Tom with those on the east in South Hadley and Granby.*" The italics

averaged 2·76 ranging from 2·71 to 2·80. That of the sandstone was somewhat less, varying from 2·45 to 2·77 and averaging 2·68.

It may be objected that, under the equilibrium hypothesis, the denser materials could not overflow the lighter, but it should be considered that the upflowing lava, on being relieved from pressure, would be likely to expand and so extend the length of the balancing column, and that its density might then in its heated condition have been considerably less than now, when cold. Moreover, its sudden flow from under the strata adjacent to the fissure would perhaps allow them to sink, so that the surface for a short distance each side of the dike would be below the equilibrium level.

It is not probable, however, that the surface of the upper strata would be kept exactly at the equilibrium level, though as already shown, geological evidence goes to prove that this was true within comparatively narrow limits, between which it is quite possible that periodical or convulsive movements occurred, owing to sudden ruptures of the strained strata. Is it not likely that such a movement would take place on the formation of a fissure, and that the reaction, on a sudden release from strain, might generate a momentum which would carry the strata down from a position a little above, to one somewhat below the equilibrium level, the difference of specific gravity, if any existed, being too small to offer but a slight resistance to the movement?

[3] (Report, p. 10.)

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# PLAN OF
# MOUNT TOBY
## MASS.

*From a Survey by* H.F. WALLING.

FORBES CO. BOSTON & N.Y.




**SCALE OF PLAN**

0 1 2 3 4 *Miles.*

0 1 2 3 4 5 *Kilometers.*




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Julius Bien Photo lith

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in these quotations are mine and I think the facts they emphasize tend strongly to corroborate the equilibrium theory.

*Dips around Mount Toby.* I made a complete circuit of this hill, close to its base, besides ascending to its summit, and took the dip at many different places. They are shown upon the accompanying plan, which, with the two sections cutting it from north to south and from east to west, will give a correct idea of its stratigraphy. It will be seen that this is quite in accordance with what was to be expected, admitting the equilibrium theory; for, although the weight of the mountain has of course been considerably diminished, so that its summit is probably higher than the upper level of the original strata from which it has been carved by denudation after its original emergence, the denudation has been so much greater all around it, that *the strata have been pushed up* so as to give them under and around the mountain the form of a basin or canoe. The eastward dips, however, predominate, as might be expected from the general inclination of the basin.

*Sections by Dr. Hitchcock.* The sections by Dr. Hitchcock,[4] a reduced copy of which, and of that portion of the map showing Massachusetts territory, accompanies this paper, indicate a similar pushing up of the strata wherever the denudation has been greater than in its neighborhood. Thus in the vicinities of Mt. Tom and the Holyoke range, preserved by the hardness of the trap of which they are mainly composed, we find that the dips on the east and south, as already stated on Hitchcock's authority, are not so great as on the west and north, or, that the general eastward inclination has been increased on the west and diminished on the east, as might be expected in consideration of the weights of these elevations resting upon a bending crust supported by a semi-fluid foundation. The westerly dips of the sandstone against the granite hills on the west may, in the same way, indicate the superior resistance of the latter to denudation.

*Bending of the Crust by the weight of Sediments.* If it should seem mechanically improbable that a weight comparatively so small as that of a hill 1000 feet in height should be able to bend down a crust of rock many miles in thickness, it must be remembered that this same region affords an example where the crust

[4] Hitchcock's Report on Ichnology.



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yielded gradually to small increments of weight throughout the whole 8000 feet or more of its deposition.[5] Somewhere between the solid crust at the surface and the region where the intensely heated matter is rendered permanently rigid by the unremitting and tremendous pressure of the weight above, must there not be a region, which if not plastic, is rendered so by the removal of a portion of the pressure from any direction? If, as supposed by G. Poulett Scrope[6] and advocated by T. Sterry Hunt and others,[7] plasticity is produced or greatly augmented at a temperature much reduced from the point of fusion, by the presence of water among the heated rocks, may not the upper limits of plasticity be brought so near the earth's surface as to render the crust above sufficiently flexible to yield, though perhaps with extreme tardiness, to such inequalities of pressure as are here exhibited? Moreover, have not the experiments of Tresca upon the flowing of metals, those of Wöhler and Spangenburg upon the "Fatigue of Metals" under repeated strains, and of others upon the nature of "permanent set" and the imperfect resilience of solids in general, proved that the difference between known solids and liquids consists in the amount rather than the presence or absence of a resistance to change of form, and in the length of time required for internal strains to relieve themselves by effecting the deformation that they tend to produce? May not the question of the resumption of the equilibrium figure by the earth's mass, after it has been disturbed by denudation, be simply one of time? And, since the process of denudation is itself so exceedingly slow, may not the restoration go on with the disturbance, so far, at least, as an average contour of surface is concerned?

*Flexure caused by Unequal Denudation.* Let us suppose these questions answered in the affirmative, and that the Green Mountain Range was originally a plateau, having a height in this latitude, of from 300 to 500 metres, with more or less gentle east and west slopes, the somewhat reduced density of its underlying material, as compared with the adjacent regions, causing it to float up above them. During long geological periods it has accordingly been subjected to denudation, and as fast as its surface was lowered by

[5] James Hall, Paleontology of New York, Vol. III, p. 69. J. D. Dana, Am. Jour. of Sci., 2d ser., XLII, p. 209, foot note.

[6] Jour. Geolog. Soc. of London, Vol. XII, p. 326.

[7] T. S. Hunt, Chemical and Geol. Essays, 2d Ed., p. 5.

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the slow action of rain and flood, it has regained or rather maintained its equilibrium level. The western part of the Connecticut River Triassic basin has been denuded, and hence elevated, more rapidly than the eastern, but for some reason the western flow of the viscous substratum has been so inconsiderable or so equable, across the basin, that no folding of the supported strata has been produced, such as, in general, would be likely to accompany unequal denudation.[8] More than a mile and a half in vertical thickness of rock has thus been removed from the western side of the basin where it is widest, in excess of that taken from the eastern side, without materially changing the surface level, excepting that portions less denuded, like Mt. Toby, Mt. Holyoke and Mt. Tom, have been borne up by the crust of which they form a part, which, however, has been gradually sagged downward, in a relative sense, under and around them, by their weight as we have seen. Supposing no diminution in width of ground plan to have occurred, the amount of horizontal compression produced has been only the difference between the hypothenuse and horizontal base of a triangle having an angle of elevation not exceeding 15°, say four or five per cent. of the width of the basin.

*Triassic Formation in New York, New Jersey, etc.* In the Triassic basin on the other side of the Green Mountain Range, commencing on the west bank of the Hudson, in the state of New York and extending across New York, New Jersey, Pennsylvania, etc., the general dip of the strata is across the basin towards the west and northwest in New York and New Jersey, that is, in the opposite direction to the dip of the Connecticut valley basin. This indicates an uplift towards what was probably an axis common to the two basins and lying between them. If this axis continued all the way along the easterly side of this more westerly basin, its southern extension has subsided below the level of the sea across Long Island Sound, New York Lower Bay and the state of New Jersey, probably on account of an increase of density in its underlying materials, in the manner supposed in the foregoing paper (see pp. 185, 186). Its course was probably southwesterly across the state of New Jersey, then westerly, and again bending towards

[8] If the emergence of the basin and its denudation rapidly succeeded the deposition of its strata, may not the isogeotherms, carried down by the sinking deposits, have moved so tardily on their return, as to have left the Triassic strata with the still unsoftened rocks below, more rigid and capable of resisting corrugation than those on either side?

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the south, having a general trend parallel with the Appalachian ranges in Pennsylvania, Maryland and Virginia.

*Similarity to the Connecticut Valley Basin.* The Triassic basin west and northwest of this axis resembles its Connecticut valley counterpart in having no folds, no synclinal or anticlinal axes, and in having trap outflows of considerable thickness conformably interposed between the sandstone strata. There is also a similar tendency in the direction of the dip near the trap outflows, to become normal to that of the trap ranges in their sinuosities, and a narrow belt of disturbance occurs along the northwestern border of the basin, across New Jersey, the sandstone strata being turned abruptly upwards, instead of downwards as on the west side of the Connecticut valley basin. Professor Cook describes these features as follows:[9] "Along the northwest border, and for a short distance from it, there is a steep dip to the southeast, and the rocks are much fractured. Near the trap ranges of the First, Second and Third mountains, where their direction is changed as shown on the map, from the usual northeast and southwest course to a northwest and southeast course, the dip also changes from the northwest to a northeast or southwest direction."

Now the trap outflows, which were nearly level when extruded, have, from the compactness and toughness of their structure, resisted the action of denuding agencies longer than the sandstone, and hence have remained above the general level of the latter. They have also tended by their weight to keep the strata beneath them in a horizontal position. Hence the approximate parallelism of the strike, or line of intersection of the strata by a horizontal plane, to that of the trap hills in their immediate vicinity. Thus we see that while the sandstone strata were elevated as fast as eroded, so as to maintain a nearly unchanged general surface level, hills from 300 to 600 feet in height (100 to 200 metres), which have resisted the erosion, have acted by their weight to prevent elevation in their own neighborhood, clearly proving sensitiveness in the earth's crust to inequalities of pressure.

As to whether the sharp upward flexure of the sandstone strata against the gneiss rock on the west has been produced by a more rapid denudation of these adjacent hills, causing a sudden diminution in the rate of flow of the substratum across the line of junction, by a turning against the gneiss of the upper edge of the

[9] Geology of New Jersey, G. H. Cook, State Geologist, 1868, p. 195.

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prism of sandstone, the cross-section of whose upper part is a right-angled triangle whose hypothenuse has been brought to a horizontal position by the elevation of the farther end of the formerly horizontal base, or by some other cause, I will not venture to express an opinion.

*Relation of the Earth's Exterior to its Interior.* The examples presented above, which I have no doubt are similar, in the lessons to be derived from them, to many others which could readily be cited by geologists, seem to show that the interior of the earth maintains close relations with its exterior, and that the "grand generalization of Humboldt," according to which all geological phenomena may be fused into one idea of "*the reaction of the interior of the earth upon its exterior*,"[10] is more directly and comprehensively true than Humboldt imagined. The evidence becomes more and more conclusive that the force through which the reaction is produced is simply that of gravity. Even the energy displayed in volcanoes and earthquakes has been attributed, and doubtless with truth, to disturbances of gravitational equilibrium by Herschel, Babbage, and later by Sterry Hunt. The view of Mallet that the heat energy of volcanoes has been derived from a conversion of the falling force of a collapsing shell, however ingenious, is beset with difficulties as to the mode of localizing the energy, etc., while if we have a heated substratum subjected to heavy pressure and thus maintained in a state of repressed liquidity, a partial removal of the pressure is sufficient to bring about upward or lateral flow of the heated material and the other consequences which naturally follow, producing, under certain circumstances, volcanic eruptions.

If we admit that the earth's interior, when its gravitational equilibrium is disturbed by surface movements, reacts upon the surface in the manner which I have supposed the stratigraphy of the Appalachian Triassic Region to indicate, we have, in the *present topography* of the earth, data from which new light may be thrown upon the geological dynamics of the past, while on the other hand the stratigraphy of the rocks will give much information in regard to the archæology of physical geography, or "paleotopography."

[10] Whitney on Earthquakes, etc., p. 7.

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## On the Complete Series of Superficial Formations in Northeastern Iowa. By W. J. McGee, of Farley, Iowa.

This paper comprises a brief summary of the results of several weeks' examination of the superficial geological formations of parts of Dubuque, Delaware, and Clayton counties, Iowa. Not less than 800 miles have been travelled over, and 200 sections examined more or less carefully. The examination of over 500 square miles has been completed, and materials collected for the preparation of a full series of maps and sections illustrative of the topography and the geology, both of the older rocks and the surface deposits, of the region. The work has been done on private account in connection with archæological reconnoissance, also undertaken privately, though with some expectation that the results will be finally made use of by the state. No publications of any character have heretofore been made embracing any of the data collected or observations made, except a very brief paper in the *American Journal of Science and Arts*.[1]

The drift in this region is clearly resolvable into five members, any of which may, however, be absent. These are, in descending order, (1), a perfectly homogeneous, loess-like clay, reddish-yellow in color, and quite free from bowlders, gravel, and sand; (2), a stratum of similar clay usually mixed with gravel and sand, and closely packed with small sub-angular and rounded bowlders; (3), a clay usually darker in color, throughout which pebbles and bowlders, often large and angular, are scattered promiscuously. It is sometimes rather arenaceous in structure, and occasionally exhibits imperfect stratification, beds of pure clay alternating with those of sand or gravel; (4), a blue or black clay, nearly always free from bowlders, sand, and gravel, and usually containing wood or other organic substances in a more or less perfect state of preservation; and (5), a yellow clay much mixed with, and sometimes replaced by, sand and gravel, always containing many small, metamorphic bowlders. This member invariably rests on the older rocks when it is present. For convenience of reference these members are designated by their numbers, in the above order, throughout this paper. Their relations will be better understood from an examination of the accompanying general section.

[1] "On the Relative Positions of the Forest Bed and Associated Drift Formations in Northeastern Iowa." *Am. Jour. Sci.*, III, Vol. XV, No. 89, May, 1878.

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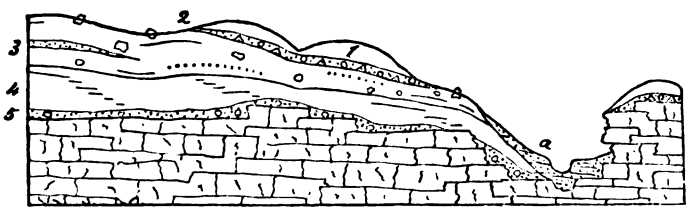
A more detailed description of each member will be necessary in order that their character and relations may be fully comprehended.

Number one is very variable in thickness, and is quite frequently absent. It may even appear only on the brow of a hill, being absent both on the hill-top and in the valley—in the first-named situation evidently because it never was deposited, and in the valley because it has been removed by erosion. Twelve or fifteen feet is probably about its average thickness, with an extreme of perhaps forty or fifty feet in the eastern part of the region described, along the Mississippi. It is quite loess-like in structure, being perfectly homogeneous from top to bottom, and never show-

Fig. 1.




General Section exhibiting succession of strata in the superficial formations of Northeastern Iowa. *a*. Alluvium, found in "bottoms." 1. Loess-like clay. 2. Sandy and gravelly clay. 3. Glacial drift. 4. Lower till—blue clay. 5. Sandy and gravelly clay, resting on Silurian strata.

ing lines of stratification or lamination so far as observed,[2] though it sometimes exhibits a jointed structure when freshly exposed by heavy rains. This structure permits it to "wash" readily. Hence its presence is characterized by numerous narrow and steep-sided ravines, running towards every point of the compass, the intervening hills being frequently so narrow and steep as to materially depreciate the value of the land in an agricultural point of view. This peculiarity is strikingly noticeable only where the stratum is quite thick, and the subjacent sedimentary strata but slightly eroded and cut into valleys, as in parts of New Wine and Vernon townships in Dubuque county.

[2] On river bottoms, and especially near the mouths of small tributaries to the Mississippi, this member may often be seen mixed with sand and gravel and very distinctly stratified. It may also be laminated, the lamination being usually oblique to the lines of stratification. In such cases the laminæ do not present straight, but slightly curved lines, when seen in cross-section. Whenever such a modification is observable, however, the character of the formation is such as to indicate clearly that its materials have been reassorted by alluvial agencies since their first deposit.

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This member forms, consequently, a very prominent topographical feature wherever found. Indeed its presence is more readily noted by the change in the general features of the surface on entering its territory, than by the absence of bowlders, and the homogeneous structure of the clay; and this by the careful geologist as well as the casual observer. An actual section through a region of this character shows the hills to present more or less irregularly convex surfaces, the convexities being so crowded together as to leave only V-like depressions to represent the valleys. A similar section through a region covered by unmodified drift (member number three) exhibits, on the contrary, a wavy surface line, the hills being represented by convex and the valleys by tolerably regularly concave surfaces. The hills are relatively higher, also, where member number one forms the surface.

In all that part of the Wisconsin driftless region which extends into Iowa,[3] this member forms the surface, as well as for some distance beyond its borders. It gradually runs out, however, and is not found except in isolated cases on the level drift prairies farther to the westward. Whether or not the soil of this member is unusually favorable to the production of forests, I am unable to say; though it is certain that in the natural state it is usually covered with timber, while the adjoining drift areas have no covering naturally, except of grasses and small plants. It is certainly more favorable to the growing of fruit trees than the soil of member number three. It has long been noted by horticulturists as a curious circumstance, that while apple-trees, especially, flourish finely in the immediate vicinity of Dubuque, they can only be coaxed to maintain a sickly existence by means of the greatest care, a few miles farther westward, where the surface is covered by unmodified drift. This unfavorable tendency of the soil of number three certainly must be counterbalanced by more favorable climatal conditions in other localities, as orchards flourish on soil of this character in many places. The presence of a thick sod and the vigor of the accompanying grasses may have something to do with preventing the germination and proper nutrition of the seeds of forest trees, in a state of nature; but it is hard to see how this could affect the production of fruit trees in cultivated land. However that may be, the surface of number three is thickly covered with a tough sod, which does not entirely decompose

[3] "Geology of Wisconsin," 1877, Vol. II, p. 608, *et seq.*

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until some years after the land is broken, while number one never has a thick or tough sod, even when uncovered by timber; though this rarely occurs. So generally does the presence or absence of forests accord with that of the loess-like clay, that many agriculturists look upon bowlders as a nuisance necessarily accompanying prairie farms in order to preserve a "balance of advantages" between them and the farms from which the timber has to be removed.

A slight difference in the relative productiveness of grain in the soil of the two members may also be observed. Wheat yields more abundantly, and failures are less frequent when sown on the surface of number one, while corn is noticeably better on that of number three. To what extent these observations bear upon the question of the treelessness of the prairies must be left to future inquiry.

Member number two of the series is almost invariably found directly below number one, being but very rarely absent when that is present, or present when that is absent. Ordinarily, from one-fourth to three-fourths of its bulk is made up of fragments of chert, small bowlders and pebbles of both metamorphic and sedimentary rocks, gravel, and some sand. The metamorphic bowlders and pebbles are from the archæan beds far to the northward, while the whole and broken nodules of chert and the bits of limestone are from the Silurian strata of the immediate vicinity. The bowlders are small, rarely more than 100 pounds in weight, and are much water-worn, though but seldom striated. The thickness of this member will average perhaps from two to four feet, though it varies considerably. It usually passes by insensible gradations into the next member, so that its exact thickness is difficult to determine.

These two members are simply modified forms of the glacial drift. The upper member consists of the lighter, and the lower member of the heavier materials forming the drift, assorted by the action of moving water during the Champlain period, or during the final recession of the glacier. Their relations will be more fully discussed later.

Member number three is unmodified or slightly modified glacial drift, and is distinguished by the same peculiarities here as elsewhere.

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Bowlders of the archæan rocks are abundant and quite large, frequently reaching a diameter of eight, ten, or even twelve feet. Granite is the predominating material, though quartzites, argillites, and other classes are represented. The granite is usually porphyritic (with feldspar), and is rarely schistose or gneissoid. Though quite hard, the surface disintegrates slowly and scales off when exposed to the action of the elements. Ramifying veins containing a larger proportion of quartz, and hence not so rapidly disintegrated, sometimes cause the bowlders to weather into odd and fanciful forms. Rudely spherical or angular masses of granitic materials, in which there is so little cohesion not only between the grains but even between the particles forming the crystals, that the spade may be driven through them as through so much sand, are occasionally found many feet below the surface, in excavating wells. They are obviously disintegrated granite bowlders, and the disintegration must have taken place since the glacier. Bowlders have been so abundant in some fields that a continuous strip a rod wide, around a forty-acre lot, may sometimes be seen, formed of those taken from that lot alone.

*Kames*, *eskers*, or *asar*, singly or in groups, are frequently met with. Owing to their nature their materials have been assorted and modified by aqueous currents, and their upper strata are frequently members number one and number two in their normal positions; though sometimes they are composed wholly of gravel. *Roches moutonnées* are common also, though not high and therefore not prominent features in the landscape. The *stoss* side cannot always be determined. Imperfect stratification is sometimes exhibited throughout member number three, though generally there is no nearer approach to stratification than local beds and lenticular patches of sand or gravel. The bowlders near the surface seem to be larger than those below, where there is also a larger proportion of limestone bowlders from the immediate vicinity,—though these are always rare and frequently altogether absent. Striated bowlders of Niagara limestone have been observed near Delhi, Delaware county, where they were thrown from a railway cutting. In one case a much worn fragment of Trenton limestone containing many shells of the Trenton Brachiopods, *Orthis* and *Strophomena*, was picked up in the northwestern part of Dubuque county. In order to reach this position it must have been transported by the glacier at least forty or fifty miles, and

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probably much farther. It must have come from the valley of the Turkey river, far up towards its source. Striated bowlders of the metamorphic varieties are singularly rare, probably not more than one in a thousand being so marked; and I had examined many thousand before I observed the first instance in this locality. Such as have been seen (if the small bowlders and pebbles from members four and five are excepted) are invariably in the immediate vicinity of outcrops or *roches moutonnées* upon which they may have been ground. Striated rock-ledges are unknown, owing, doubtless, partly to the somewhat friable nature of the Niagara limestone, which forms the foundation on which the superficial formations of most of this region rest, and partly to a cause which may be brought to light later. Nodules and broken fragments of chert from the Niagara limestone form a conspicuous feature of this member, especially near its base.[4]

Striæ being absent, it is somewhat difficult to determine the direction of the ice-flow during the glaciation of this region. The shape and direction of *kames* and *roches moutonnées*, and their relative positions, afford the only accessible means of solving this question. Though the *kames* are sometimes approximately circular, they are generally oblong in shape; and in either case they are not usually isolated but are connected in ranges of two or three or even more,—sometimes a dozen, extending over nearly as many miles, with scarcely a break. It is found that the direction of these ranges and the transverse diameters of the elliptical *kames* correspond, both extending very nearly S. 75° E. No variations of more than from two to five degrees from this direction have been observed, except in the case of small ridges and hillocks in broad valleys, obviously formed during post-glacial times. This then must have been, approximately at least, the direction of the slope of the surface of the ice-sheet (assuming the formation of the *kames* to have occurred as described by Prof. Warren Upham, Assistant State Geologist of New Hampshire, in the *American Journal of Science* for last December (1877), in Vol. III of the final reports of the Geological Survey of New Hampshire, and previously in the Proceedings of this Association, Vol. XXV, p. 216, *et seq.*); and as this corresponds quite closely with the

[4] The disintegration of the Niagara limestone, leaving great quantities of nodular chert, or hornstone, is accurately described by Prof. James Hall in "Geology of Iowa," 1858, pt. 1, p. 71.

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general slope of the country, it must also indicate pretty nearly the direction of the glacial motion. The *roches moutonnées* confirm this, as their *stoss* side (when that can be determined) faces the opposite point of the compass; and they usually extend longitudinally in the same direction as the *kames*.

Though terraces are frequently found along the rivers and smaller streams, formed of this and other members of the series, they are invariably such as have been formed by the action of the streams themselves in alternately corrading and filling up their beds, as they shifted from side to side of their valleys. None have been observed which afford the slightest evidence of the occurrence of continental oscillations since the deposit of this member, or even since the laying down of the sedimentary strata. Not even a gradual elevation of the surface is required as one of the factors in the formation of such terraces as those of this region.

As before stated, this member forms the natural surface over the greater part of Iowa. Its exact junction with the bowlderless territory has not been fully determined, owing to the extreme difficulty attending the investigation. The difficulty arises from the fact that this member is deeply covered everywhere near its eastern limits, by members one and two. Were complete sections exposed near the junction, the character of the deposits could, of course, be readily determined; but this is so seldom the case that I have ceased to rely on this means. Natural sections would indeed be common where both deposits are exposed to the erosion of streams, were it not that the incoherent and pulverulent nature of member number one permits it to form a talus burying all beneath it. I have, therefore, been compelled to depend upon the occasional presence of bowlders exposed by freshets in the deeper drift valleys, and the absence of slender pinnacles and fragile peaks of limestone, as a means of determining the limits of the glaciated area. The presence of bowlders, unless they are quite large, however, cannot be depended on as an infallible guide, where the country slopes so rapidly—525 feet within twenty miles—from the glaciated and over the bowlderless region toward the river, as there is a strong probability that some have been transported by water. Hence the only perfectly reliable evidence of the limits of glaciation is the presence of slender peaks on one side and their absence on the other. These are so rare that I have

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thus far made no effort to determine the exact junction, only recording the position of the unsupported rocks as observed.

This member is frequently denuded of the superincumbent deposits so that its exposures seem to follow the water courses. This is well exhibited in Cascade, Liberty, and Taylor townships, in Dubuque county. In Dodge township, on the contrary, where but a thin stratum of loess was deposited and the water courses have but little fall, member number one tends to follow the streams. This anomalous distribution necessitates great caution in fixing the limits of the several members on maps and sections.

The average thickness of this deposit may be put at twenty-five feet, though it is very variable. Its base is not horizontal, as might be supposed, but is to a remarkable extent conformable with its surface, which had also been eroded before the deposit of members one and two. Hence if it were removed bodily the general topography would not be materially changed. To a less extent the same may be said of the two subjacent members. The sedimentary strata have been corraded and excavated into ravines and valleys which correspond quite closely with those of the present surface.

The presence of the unmodified glacial drift over the surface of a large part of the state was recognized by Dr. C. A. White, the last State Geologist, and it was by him described at some length.[5] He seems to have erred, however, in placing the eastern limit too far west, as he describes Dubuque county as being destitute of bowlders;[6] but such an error is not astonishing when we consider the cursory and imperfect examination he was compelled to make by the provisions of the act authorizing the survey, and does not detract from his capabilities as a geologist.

Member number four of the series is a fine clay, or more properly a combination of finely comminuted sand, and perhaps marl, oxide of iron and other agents in small quantities, with a sufficient admixture of silicate of alumina to form a stiff and tough mass. Its color varies from a bluish white to a deep black, the prevailing color, where not weathered, being a deep blue. Its coloration is due to the presence of protoxide of iron in varying quantities. It is sometimes finely laminated, sometimes irregularly stratified with intercalated layers of coarse sand or of differently

[5] "Geology of Iowa," 1870, Vol. I, p. 82, *et seq.* [6] *Loc. cit.*, p. 87.

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colored clay, and sometimes exhibits no trace of the action of moving water, in which case — as well as in the purer layers where stratified — a well marked jointed structure has been observed. This is not a constant feature, however. Parts of it are occasionally so indurated as to resemble shale in structure and consistency, as in wells No. 39 (see Table I) and 47–49, where it was removed with great difficulty by means of pick and crowbar, and in well No. 40, where the laborers drilled and blasted it, mistaking it for rock. Fragments of this shale are often highly bituminous, so much so as to be imperfectly combustible, as in wells No. 51, 53, and 59.

Southwest of Colesburg, Delaware county, the clay of the lower part of this member is so pure as to yield fine pipe clay. In this village is a pottery, the material used in which is brought from the Little Turkey river, two miles to the eastward. The formation from which it is taken is the Maquoketa Shales of Dr. White,[7] the lower portion of which, there as at many other localities, consists of a fine blue clay. The proprietor of the pottery has tested the clay from our member number three, however, and finds it superior to the really excellent article from the shales. He is only deterred from using it by the great expense of bringing it to the surface. It is considered by the potters to be a singular circumstance that the clay which they use should be found in one place above, and three or four miles distant, below a solid, heavy-bedded limestone (Niagara). I can testify that it is frequently difficult to distinguish the materials composing these two formations so widely separated geologically — though here sometimes almost associated — especially where both consist of a bituminous shale overlying blue clay, precisely similar in appearance and consistency in both.[8]

In one case (well No. 19) the blue clay, member number four, was found only in one side of the well, the other being occupied by number three. Number four seemed to be pressed out of shape and contorted as if by pressure, in a manner imperfectly shown in the accompanying section. Analogous sections are figured by

[7] *Loc. cit.*, p. 180. Dr. White considers this formation to have been laid down during the same period as the Cincinnati group, though representing a distinct epoch. It is the same formation as that designated *Hudson River Shales* by Prof. James Hall, in 1858 ("Geology of Iowa," 1858, pt. 1, p. 64), and is the highest member of the Lower Silurian system found in Iowa.

[8] The carbonaceous nature of the Maquoketa Shales is well shown in the analyses of specimens recorded in Hall's "Geology of Iowa," pp. 358–361.

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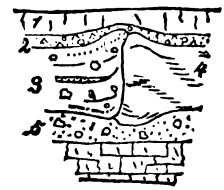
Geikie, the Scottish Geologist, in his "Great Ice Age."[9] This section will be referred to again.

Besides the bituminous shale already adverted to, which is found in this stratum, the whole deposit is highly nitrogeneous and carbonaceous. When—as occasionally happens—it is exposed in sloughs and valleys by the removal of the overlying members, the soil is extremely fertile; and when the clay has been removed from wells and spread over the surface, it acted as an excellent fertilizer. This was especially noted in the case of well No. 28, where the blue clay was spread around the roots of some sickly fruit trees, with most satisfactory results. Generally, however, it is only the upper part of the member which is thus rich in nitrogen. Organic matter in the shape of wood, etc., is found in about half of the wells in the region described. When exposed on hill slopes and in valleys formed in the unconsolidated deposits alone, small springs issue from the surface of this clay. The water, percolating through the more porous materials of the superincumbent masses, collects on the surface of this nearly impervious stratum, and gradually makes for itself a channel communicating with the external water courses.

Fig. 2.




1. Loess-like clay, 4 ft.; 2. Yellow gravelly clay, 2 ft.; 3. Glacial drift, with rounded and angular bowlders, 8 ft.; 4. Blue clay, with distorted lammæ, 8 ft.; 5. Yellow clay, with much sand and many well-rounded pebbles and bowlders, 8 ft.

The wood found in the wells is in all states of preservation. Sound logs so slightly mineralized as to burn readily form one extreme, and roots and branches which may be rubbed to powder in the fingers, the other. I have seen an elm trunk from which basket-splints might have been made. Slivers could be wrapped around the finger, and, on the pressure being removed, would spring back to their old form like whalebone. Generally the wood is of the varieties now found growing upon the surface; the only difference yet noted being in the abundance of coniferous logs and fragments in the deposit, and their comparative absence in the existing forests. With some reservation it may be said that remains of vegetation occur indiscriminately in all parts of the blue clay, or even in the gravelly stratum below it. When there is *a total absence of stratification*, the wood is found only at or near

[9] *Op. cit.*, pp. 144, 145. See also "Climate and Time," Croll, p. 465.

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the surface of this member; but this is not very often the case in this region. In the majority of cases it occurs only in fragments, though there are numerous exceptions. In well No. 36 a stump was found at a depth of thirty-four feet, so large and well-preserved that after working two days and removing "bushels" of chips, the laborers were compelled to abandon the well. In well No. 30 a log eight inches in diameter and quite sound, extended across the well and was cut off at both sides. In well No. 17 a similar log was found, too large to be removed. In No. 54 one side of the well intersected a stump too large to be removed, which rendered the water unfit for potable uses when deep enough to reach it. In No. 26 "sticks of cord-wood" were found, which were declared by those who saw them to still retain axe-marks on the ends. They were cut up and burned in the kitchen stove. *Apropos* to the finding of axe-marks, "chips" are reported to have been found in many instances. Similar "chips" and "axe-marks" have been attributed by Dr. J. S. Newberry, whose ability is unquestioned, to the action of the teeth of *Castoroides ohioensis;*[10] but the same cannot be said of the following "finds." From well No. 59 a piece of timber six inches in diameter and *accurately squared* was reported, from a depth of twenty-three feet. A piece six feet long, evidently from near the prow of a canoe, hewn on the outside and burned out on the inside, is reported from a well (not in the table) in section 18, Iowa township, Dubuque county. It was found thirty-five feet from the surface. A short piece of log ten inches in diameter, burned off at one end and perforated by a perfectly straight and regular round hole an inch and a quarter in diameter near the other, is reported from well No. 68. It was found at a depth of nineteen feet.[11] A yet more striking case

[10] "Geological Survey of Ohio," pt. 1, Vol. II, p. 32, note.

[11] This case has been added since the reading of the paper. The genuineness of the "discoidal stone" spoken of in the next sentence, has been peremptorily denied by one archæologist and questioned by others. Thus far, however, there is as much evidence, so far as the authority of noted specialists goes, in favor of, as against, its genuineness. My own conclusion as a geologist, and not as an archæologist (as will presently appear this is not yet a question for the consideration of archæologists) is, that it is not a naturally formed pebble. My competency as a judge of such matters may be questioned. I can only say that after having examined hundreds of tons (to keep far within bounds) of water-worn pebbles, I have never seen one at all resembling this, or even one in which the same principles were involved in its fashioning. The specimen presents two *parallel* plane surfaces (at one point there is a slight flaw and depression on the larger surface), and its periphery presents a *circular* outline. The periphery, when viewed edgewise, presents an irregular convexity, but this convexity is precisely similar at whatever point the specimen is examined. The geologists to whom I have

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was the finding in the same horizon, at a depth of thirty feet, of a "discoidal stone"—a partially polished implement probably used as a hammer. This is reported to have been found in a well in section 1, Colony township, Delaware county. There are also many well-authenticated cases of the finding of charcoal and charred wood. Some such have come under my own observation, and I can therefore vouch for them. Archæologists will probably regret, however, that so little value can be attached to any of these "finds." The squared timber and the fragment of canoe were both inspected by several perfectly reliable parties, but all of them wofully ignorant of even the merest rudiments of geological and other sciences, and with no conception of the limitation of the senses, and the proneness of imagination to fill up what is lacking in reality. After such careful examination the specimens were permitted to become lost or destroyed. Hence, while their authenticity could be unequivocally established before a court of record, the necessary conservatism of archæology prevents their acceptance. The same may be said of the perforated block, which fell a victim to the house cleaning epidemic, two years after its discovery. Several independent witnesses have described this article to me, and detailed the circumstances under which it was found, and their accounts exactly correspond; and all were so unskilled as to be perfectly unbiased. The perforation may have been produced by natural causes though, as is quite possible also as regards the charring of this and other specimens; consequently, such cases are not positive proof of the agency of man.

Dr. Oscar Peschel, the German anthropologist, enumerates some of the natural means by which fire may be generated in his "Races of Man;"[12] and a case is mentioned by a writer in *Nature*, in which an Algerian forest was fired by the sun's rays refracted in passing through raindrops suspended from twigs. The discoidal stone is now in my possession; but it has been used on occasion for the last thirty-three years, to drive nails and crack nuts, and of the two men who exhumed it, one has been dead for twenty-five years, and the other is in a distant territory where his address is

submitted it have, with one doubtful exception, concurred in pronouncing it *not* a water-worn pebble. Since archæologists have raised the question, however, it cannot now be settled without the aid of the most distinguished specialists, in this branch of science, now living.

[12] *Op. cit.*, pp. 136–144.



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unknown. Hence, inaccuracies may have crept in sufficient to vitiate the case.

It is not by any means intended that these instances shall be looked upon as evidence of the contemporaneity of man with the ancient forests whose remains we find so abundantly in this stratum; but a geologist does not perform his whole duty who neglects to record data because they do not happen to verify other observations or strengthen existing generalizations. Accordingly these contributions to the literature of our surface geology are made in the hope that they may prove of value in the future, but without the least inclination to magnify their intrinsic value or to add to their apparent weight by disregarding depreciatory elements. Relics of man have indeed been found in Europe in what is undoubtedly the precise equivalent of this stratum, and by Dr. C. C. Abbott in the unmodified glacial drift of New Jersey;[13] and though it may ultimately be proven that the expectation is but an *ignis fatuus*, I have no doubt that when our surface deposits are as thoroughly worked up as those of many parts of Europe, vestiges of humanity will be brought to light from the American "forest bed." If they are, observations now apparently valueless would aid in elucidating the social history and intellectual *status* of the race to which they pertained.

Pine, cedar (these varieties are not now found in the vicinity except in isolated cases along the river bluffs), oak, maple, elm, linden (basswood), hickory, willow, and sumach, have been recognized from this member. I have also been shown a pipe made by a German farmer from a "sweet-brier" found coiled around the trunk of an oak exposed in excavating a reservoir for a spring, in section 1, New Wine township. The wood is always more or less stained and permeated by oxide of iron, and generally so far mineralized as to be incombustible. Traces of hydrated per-oxide of iron are frequently found in this stratum. It often occurs in the form of cylindrical concretions formed partly of clay stained brownish by the iron, exhibiting concentric lines when viewed in cross-section. These are sometimes hollow, and sometimes contain the rootlets around which they were originally formed; and they frequently display the gnarling and ramifica-

[13] "On the Discovery of supposed Paleolithic Implements from the Glacial Drift." Tenth Annual Report of the Peabody Museum, p. 30, *et seq.*

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tions of the roots. These were found in many of the wells in which wood was not noted. In one instance, three-fourths of a mile northwest of Farley, impure bog-ore has accumulated to a considerable thickness. The bed has been excavated to a depth of four feet without penetrating it.

In two instances, coarse pebbles were found scattered throughout this member. Investigation showed that in each of these cases the blue clay rested directly on the limestone, without the intervention of member number five. It has also been observed that when the member is unstratified, the wood is found on or near its surface. When stratified it may occur, as before stated, anywhere within it or even below it. This member is found in about two-thirds or three-fourths of the wells in the region described. (No record was kept of those wells in which nothing of interest appeared in the stratigraphical sequence, and hence the exact proportion is unknown.) The average thickness of the stratum may be put at twenty feet, though it varies from a mere trace to sixty feet (in well No. 42), the greatest thickness observed in Dubuque county, or even 100 feet, as in well No. 66, in Endfield, Clayton county. This member encroaches further upon the driftless region than number three. It has been found containing wood within five miles of the Mississippi at Dubuque, as in well No. 60, observed by J. T. Everett, C. E. This is some miles to the eastward of the limits of number three. I have also seen exposures of the blue clay on Catfish creek, less than three miles from its mouth, and the same distance from Dubuque; but it may here have been transported from farther to the westward.

Member number five is generally unimportant in dimensions, though of considerable practical importance, as within the sand and gravel composing it an abundance of good water is usually found. As before stated, its absence has been noted in but two instances when number four was present, though it is seldom thick. Perhaps four feet may be taken as its average thickness, though it ranges from a few inches to fifteen feet. Its color is usually reddish-yellow, though it sometimes shades into a dark reddish color. Frequently the gravel within it is stained externally, and for a little distance beneath the surface, by oxides or other agents forming part of the bulk of this member.

Its individuality is clearly shown by the character of its bowlders

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and pebbles. Hornblende is a predominating material, and syenite, diorite, porphyry, hæmatite, hornblende schist, and hornblendic slate, are common varieties. All of these varieties are rare in members two and three, though found in member number four in the two cases in which it was stony. There are other differences in the mineralogy of members three and five which experience has taught me to readily recognize ocularly, but which I am not able to define intelligibly, owing to imperfect knowledge of the lithological characteristics of the metamorphic rocks; and I have been deterred from attempting to acquire a critical knowledge of the lithology of these bowlders by the magnitude of the undertaking. A score of varieties of pebbles, or even twice or thrice that number, may frequently be seen scattered over an outcropping ledge of limestone within a radius of twenty or thirty yards. They are most abundant of course, where the pebbles and bowlders of both members three and five lie intermingled. Fragments of chert and limestone are also found in this member.

The accompanying Table I contains actual sections at the several localities named, and gives a good idea of the thickness and variations of each of the members, as well as the frequency of finding wood, etc. Many are taken from personal observation, several from practical well-diggers who remember or record the stratigraphy as a guide for future operations, and some from proprietors who possess definite information concerning the strata passed through. All doubtful cases have been rejected; and hence all those which are recorded in the table may be relied on as authentic. Table II shows approximately the relative abundance of the various pebbles in the five members taken together. The figures were obtained by counting the pebbles of the various kinds in a measured space on a creek-bottom in section 32, Iowa township, Dubuque county, where the five members are present. It is probably a fair average showing of the comparative abundance of the pebbles. Those marked with a dagger are probably from number five, those with an asterisk from number three, and those unmarked may be from either.

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# TABLE I.

## TABLE OF SECTIONS (WELLS) IN THE DRIFT OF NORTHEASTERN IOWA.

*The sign — indicates that the stratum was not penetrated; + that rock was reached; × that wood was found; and = stratification.*

| NO. | COUNTY. | TOWNSHIP. | SECTION. | PART OF SECTION. | OWNERS' NAMES. | THICKNESS OF MEMBERS. 1 Loess-like clay. | 2 Clay with much gravel and sand, and bowlders. | 3 Unmodified Drift. | 4 Blue clay with organic remains. | 5 Sand, gravel and pebbles. | TOTAL DEPTH. FEET. | REMARKS. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | Dubuque. | Liberty. | 36 | S. E. N. E. | JOHN RIES. | | 20 | | × 5 | 2— | 27 | Large bowlders in 5. Water [abundant. |
| 2 | " | " | 31 | S. E. N. W. | H. BOHNENKAMP. | | 15 | | × 5 | 4— | 24 | 5 was sand. Wood, pine. Water [abundant. |
| 3 | " | " | 31 | " " | " " | | 10 | | × 4 | 2— | 16 | 5 sand. Pine stem 5 in. thick. Water [abundant. |
| 4 | " | " | 30 | N. W. S. E. | HENRY KLAUS. | 8 | 9 | | × 19— | | 36 | No water. Wood in fragments. |
| 5 | " | " | 30 | " " | " " | 6 | 9 | | × = 25— | | 40 | Water. 25 feet lower than well No. 4. |
| 6 | " | New Wine. | 3 | N. W. N. E. | ANTON KLAUS. | | 26 | | × 1— | | 27 | Log 5 in. thick, not identified. No [water. |
| 7–16 | " | " " | 3 | " " | " " | 20 | 4 | 16 | trace | 19 | 59 | Average of 10 wells. No water in [any. |
| 17 | " | Liberty. | 19 | S. E. N. W. | HENRY PASKER. | 8 | 2 | | × = 27 | reached. | 37 | Log too large to remove, and 'chips.' [Water abundant. |
| 18 | " | " | 19 | N. E. N. E. | J. D. HAGER. | | | 10 | 4— | | 14 | 4 indurated, shaly. Water. |
| 19 | " | New Wine. | 24 | N. W. S. W. | RICHARD BARRY. | | | 14 | 6 | 10+ | 30 | No Water. |
| 20 | " | " | 24 | " " | " " | | | 12 | 8 | 6+ | 26 | " " |
| 21 | " | " | 23 | S. E. N. W. | JOHN HINDS. | | | 6 | = 14— | | 20 | Water. |
| 22 | " | " | 23 | N. E. S. W. | " " | | | 16 | trace | 4+ | 20 | No Water. |

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# TABLE I. CONTINUED.

| | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 23 | Dubuque. | Iowa. | 21 | N. E. N. E. | J. COLEMAN. | | | 25 | 25 | 2— | 52 | Water. | Wood at junction of 4 & 5, |
| 24 | " | " | 17 | S. W. S. E. | L. DUGGAN (per W. I. Anderson). | | | 25 | 20 | 3— | 48 | " | Traces of wood, decayed. |
| 25 | " | New Wine. | 12 | " " | GARRETT COTTER. | 15 | 3 | 12 | 20 | 1— | 51 | " | Fragments of wood. |
| 26 | " | " " | 12 | " " | " " | 5 | 4 | 8 | 20 | 2+ | 39 | "Sticks of cord-wood." | 40 ft. lower than well No. 25. No water. |
| 27 | " | " " | 1 | N. W. N. E. | GEORGE GLASS. | 6 | trace | 30 | 3— | | 39 | Water. | |
| 28 | " | " " | 11 | N. E. S. E. | H. OBERBROECKLING. | | 40= | | 10 | 1— | 51 | " | 4 used as a fertilizer. |
| 29 | " | Liberty. | 24 | " " | ANTON DUSTERT. | | | 20 | 14— | | 34 | " | |
| 30 | " | " | 15 | S. W. S. W. | ALLISON POST OFFICE. | 8 | 2 | 20 | 4— | | 34 | " | 8 inch log (und. sp.) across well. |
| 31 | " | " | 24 | S. E. N. W. | WILLIAM CLEMAN. | | | 6 | 5 | 4— | 15 | " | 5 coarse gravel. |
| 32 | " | " | 22 | S. W. N. W. | PETER GAUL. | | 20= | | 1— | | 21 | " | |
| 33 | " | " | 21 | N. E. N. E. | ALLISON CHURCH. | 3 | trace | 22 | 40— | | 65 | " | |
| 34 | " | New Wine. | 14 | S. W. S. W. | BERNARD WENTE. | | 24 | | 11 | 2— | 37 | " | 8 inch log of unknown length. |
| 35 | " | " " | 14 | " " | " " | | 24 | | 4 | 1— | 29 | " | |
| 36 | " | " " | 33 | N. W. N. E. | A. KOPPERT. | | | 30? | 4?— | | 34 | No Water. | Very large and sound stump. |
| 37 | " | " " | 14 | N. E. S. W. | BERNARD MEIS. | | 10 | | unk'n | | unk'n. | Water. | Fragments of wood and bark reported. |
| 38 | " | " " | 5 | N. E. N. E. | JOS. SCHEMMEL. | | 15 | | unk'n | | " | " | "Chips." |
| 39 | " | Taylor. | 10 | N. W. S. W. | C. R. GOODRICH. | | | 24 | 1 reached. | | 25 | " | 4 indurated, shaly. |
| 40 | " | " | 15 | N. W. S. E. | J. D. DICKINSON. | 5 | ? | 5 | 25 | 6— | 41 | " | very abundant. Many pcs. wood. Part of 4 shaly. 5 clay |
| 41 | " | " | 14 | N. E. S. E. | M. BRIGGLE. | | 35 | | 6 | 3— | 44 | Water. | and coarse sand. |
| 42 | " | Dodge. | 12 | Farley. | MARTIN DELANEY. | | | 12 | 60— | | 72 | 4 partially indurated. Wood at 60 ft. | |
| 43 | " | Taylor. | 7 | " | DENNIS MOYNAGHAN. | | | 15 | 2 | 16+ | 33 | 4 & 5 ming. at junc'n. Sand, bowlders, & rolled fossils in base of 5. | |
| 44 | " | " | 7 | " | THOS. SMITH. | | | 10 | 4 | | 14 | "Hewn" wood. Water abundant. | |

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# TABLE I. CONTINUED.

| | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 45 | Dubuque. | Prairie Creek. | 8 | S. W. S. W. | WILLIAM WELSH. | 10 | 1 | 4 | 16 | 2— | 33 | Water. Base of 4 white, indurated. |
| 46 | " | " " | 18 | S. E. N. W. | J. MCDERMOTT. | 16 | trace | 4 | 10 | 1— | 31 | " " " " " [small pieces of wood in both. |
| 47 | " | Taylor. | 11 | Epworth. | SILAS MOORE. | | 15= | | 4 | 1— | 20 | Water. 4 indurated, shaly, com- [bustible. |
| 48–49 | " | " | 11 | " | " " | | 16= | | reached. | | 16 | " 4 shaly. Average of two. |
| 50 | " | Vernon. | 33 | S. E. S. W. | P. MCLAUGHLIN. | | 20 | | 15+ | trace | 35 | No water. Bowlders in 4. |
| 51 | " | Taylor. | 19 | N. W. N. W. | N. J. LOOMIS. | | 25? | | 20? | 5?— | 50? | Water. Wood in considerable [quantities. 4 shaly, combustible. |
| 52 | " | " | 7 | Farley. | VIBBER & HEALD. | | | 29 | 14 | 3+ | 46 | I. Willow, and endogenous plants. |
| 53 | " | " | 7 | " | MCGEE & EUSTACE. | | | 21 | 10 | 4+ | 35 | II. Charred wood. Shale from 4 [combustible. |
| 54 | " | New Wine. | 36 | S. W. S. E. | W. H. HAGGARD. | | | 15? | 8? | 5? | 28 | III. Water. A huge coniferous [stump. |
| 55 | " | " " | 36 | N. W. N. E. | DAVID NICHOLS. | | | 13 | 3— | | 16 | IV. " A large stick directly [below an enormous bowlder. |
| 56 | " | Iowa. | 20 | S. W. N. W. | B. F. JOHNSON. | | | 25? | 35 | 3+ | 63? | V. Charred wood. 4 shaly. |
| 57 | " | " | 20 | N. E. S. W. | M. Q. SIMPSON. | | | 20 | 6 | 4+ | 30 | VI. Wood, sumac. 4 slightly in- [durated. |
| 58 | " | " | 31 | S. W. N. W. | FRANZ ERDMANN. | | | 10 | 20— | | 30 | Water scarce. Trace of wood. |
| 59 | " | Dodge. | 2 | S. E. S. E. | L. W. WALKER. | | | 17? | 8? | 2— | 27? | "Squared timber" at 23 ft. 4 shaly, [combustible, carbonized wood. |
| 60 | " | Julien. | 17 | " " | C. MCLEAN (per J. T. Everett, C. E.) | | | | | | — | "Chips" and other fragments at 20 [feet. |
| 61 | Jones. | 18 miles east | of | Monticello. | —— COURTNEY. | | | 40? | 58? | 2— | 100 | Water. Wood at 65 feet. |
| 62 | Delaware. | Colony. | 24 | S. E. N. W. | JAS. RETHEFORD. | 10 | 2 | 4 | 8 | 1+ | 25 | No water. 4 very hard, shaly. |
| 63 | " | " | 24 | " " | " " | 8 | 1 | 2 | 20 | 1— | 32 | Water. " " " |
| 64–65 | " | " | 24 | " " | " " | 9 | 2 | 3 | 10 | 1— | 25 | " Average of two. 4 shaly. |
| 66 | Clayton. | Cass. | 23 | Endfield. | D. & ST. P. R. R. CO. | | 30? | | 100 | | 130? | [Wood in all in fragments. 5 of No water. Well bored. [bluish peb. |
| 67 | Dubuque. | Taylor. | 34 | S. W. N W. | E. H. HORSEFIELD. | 6 | 3 | | 44— | | 53 | Many pcs. wood. 4 pebbly, seam of [sand in blue clay. |
| 68 | " | New Wine. | 22 | S. E. N. E. | G. TEGLER. | | | 20 | 4— | | 24 | Water scarce. Thick pc. of wood, [burned off at one end, pierced. |

* From *Am. Jour. Sci.* for May, 1878, where the six sections are numbered from I to VI, as above.

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# TABLE I. CONCLUDED.

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| 69 | Dubuque. | New Wine. | 24 | N. W. S. E. | J. O'CONNELL. | 15 | 7 | 6+ | 28 | No water. Log und. sp. 1 ft. thick. |
| 70 | " | Iowa. | 20 | S. W. N. W. | B. F. JOHNSON. | 9 | 2 | 9— | 20 | Small tree with twigs adhering, in [member number 3. |
| 71 | " | " | 20 | N. E. S. W | H. DECAMP, EST. | | 11 | 2— | 13 | Stick 4 inches thick at top of 4. |
| 72 | " | Taylor. | 7 | Farley. | BRYAN DONAHOE. | | 5 | 15 reached. | 20 | Water. 4 very hard, shaly. |
| 73–74 | " | " | 36 | N. E. S. W. | THOMAS HOOPER. | | 17 | 12 1½+ | 30½ | Average of two, 35 ft. apart. Lower [half of 4 white. |
| 75 | " | " | 36 | " " | " " | | 5 | 8 reached. | 13 | Water. |
| 76 | " | Cascade. | 16 | N. W. S. E. | JOHN JONES. | | 5 | 7— | 12 | In valley. 4 indurated. |
| 77 | Delaware. | South Fork. | 12 | —— | ADAM RECKER. | 3 | 5 | 15— | 23 | Good sized log. 2–3, coarse sand. |
| 78 | Dubuque. | Dodge. | 15 | N. W. S. E. | J. H. HULL. | 10 | 30 | reached. | 40 | 2–3, very sandy. Traces of wood. |
| 79–81 | Jones. | Richland. | 1 | Cascade. | BANGHART, EST. | 6 | 14 | 26 reached. | 46 | Average of 3 wells. 3 and 4 mixed [at junction. |
| 82 | Dubuque. | Liberty. | 35 | S. W. N. W. | F. STEFFEN. | | 20 | 16 reached. | 36 | |
| 83 | " | " | 35 | " " | " " | | 20 | 16 5— | 41 | |

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# TABLE II.

TABLE SHOWING THE RATIO OF THE VARIOUS KINDS OF PEBBLES IN THE DRIFT OF NORTHEASTERN IOWA.

| CHARACTER OF ROCK. | NUMBER OF SPECIMENS. | CHARACTER OF ROCK. | NUMBER OF SPECIMENS. |
|---|---|---|---|
| Flint (chert), unfossiliferous. . | 65 | Translucent quartz. . . . . | 1 |
| " " with Niagara fossils. | 24 | Yellow quartz. . . . . | 2 |
| Limestone (Niagara), without [fossils. | 3 | Coarse metamorphic sandstone. | 2 |
| " " fossiliferous. | 16 | Fine metamorphic " (one [striated). | 2 |
| Very hard dark-brown flint. . . | 8 | Coarse sandstone. . . . . | 1 |
| Hornblendic slate.† . . . . | 6 | Free corals. . . . . | 3 |
| Hornblende schist.† . . . . | 17 | Deep red "chalk." . . . . | 2 |
| Syenite.† . . . . . | 6 | Blue flint. . . . . | 2 |
| Diorite.† . . . . . | 2 | Red " . . . . . | 2 |
| Diabasite.† . . . . . | 1 | Black jasper. . . . . | 2 |
| Granite, with flesh-colored or [whitish feldspar. | 2 | Red porphyry. . . . . | 1 |
| " porphyritic.* . . . . | 6 | Catlinite (?) with impure specks. | 1 |
| " with an excess of quartz.* | 2 | Syenite, with veins of milky [quartz.† | 1 |
| " " " feldspar.* | 2 | Pudding stone of small red grains. | 1 |
| Gneiss.* . . . . . | 2 | Part of large carnelion, with inter- [ior crystals. | 1 |
| Black massive quartz. . . . | 1 | Earthy concretions, broken. . | 4 |
| Milky quartz. . . . . | 2 | Total, . . . . . | 193 |

(A few of these pebbles which I was unable to satisfactorily determine were submitted to Prof. J. D. Dana, who very kindly named them.)

That so complete a series of drift deposits should be found on the very verge of the driftless region, is certainly a striking fact, though an explicable one, and not without a parallel. Dr. J. S. Newberry records it as a "singular circumstance, that the bowlder clay is more continuous in the counties which lie along the margin of the drift area, than nearer the divide where the drift deposits are thicker."[14] The phenomenon is probably due in Iowa—partially at least—to the thinness of the ice-sheet, permitting it to

[14] "Geological Survey of Ohio," 1874, pt. 1, Vol. II, p. 25.

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pass over older accumulations without removing them, and to the low level of those parts where the loess-like clay was deposited. This will be discussed farther on.

In order to show that the more important of these formations are not confined to a limited area, as well as to assist in elucidating their true geological relations, a few brief references may be permitted.

In the first geological survey of Iowa, after its organization as a state, conducted by Prof. James Hall, of Albany, it was found that the upper portion of the drift along the Iowa river in Washington county, presented the "lithological characters of the *Loess*," though not identified with it by fossils.[15] There can be no doubt that this is the equivalent of the member number one of this paper.

An unrolled mass of silicified wood was taken from the drift in Van Buren county,[16] but its surroundings were not described; but from the fact that in all such cases where the surroundings are described they are the same as in Dubuque county, it may be assumed that all were found in the blue clay unless otherwise specified. In Lee county, "a few shark's teeth and a fragment of silicious wood" were found in such a position as to indicate that they belonged "to a period somewhat older than the drift."[17] The last State Geologist, Dr. C. A. White, records the finding of wood in several instances, sometimes "in the unaltered drift," and concludes that it was "of pre-glacial origin."[18] He also mentions the finding of peat-beds below the drift in at least two instances; one near Davenport, which is of unusual interest from the fact that mammoth remains were found associated, and one in Adair county, resting on a "dull, bluish clayey bed."[19]

Sections quite similar to many given in the accompanying table are also found in "Owen's Geological Survey of Wisconsin, Iowa,

[15] "Geology of Iowa," 1858, pt. 1, p. 241.

[16] *Ibid.*, p. 221.

[17] *Ibid.*, p. 188.

[18] "Geology of Iowa," 1870, Vol. I, p. 97.

[19] *Ibid.* — Davenport, p. 119, Adair county, p. 339. The mammoth remains were found below the member number one of this paper in probably the exact equivalent of Dr. Newberry's "forest bed," with peat and coniferous wood. The member number four of this memoir was found below, containing waterworn pebbles, some glacier-marked. Number one was well developed.

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and Minnesota." In eighteen wells in Fond du Lac, Wisconsin, the following stratification was uniformly observed:—

| | Feet. |
|---|---|
| "1. Red marly clay, | 30 to 40 |
| 2. Blue clay, with strips of quicksand, | 40 " 60 |
| 3. Sand and gravel, | 1 " 3 |
| 4. Limerock (Silurian)."[20] | |

Number one contained large bowlders. Water was found very abundantly in number three.

Charles Whittlesey describes[21] the homologue of the blue clay of Iowa as follows:—

"Fine laminated sandy and marly clay of great thickness, of a red, purple, blue, and ash color, with few bowlders and little gravel, occupying the valleys of the lakes and rivers."[22] The formation was traced from western Pennsylvania to northern Minnesota. The author adds,—"The laminated clays are invariably at the bottom, where more than one member exists," and —"There is generally a layer of gravel or sand between the drift-clay and the rocks on which it reposes. It is a thin bed of porous materials which furnishes the water to artesian wells."[23] The leaves, wood, and other organic substances found by this geologist were usually, though not invariably, from the laminated beds of clay. The superimposed deposit was invariably—"coarse sand, gravel, loam, and hard-pan, with large bowlders of northern rocks, occupying the surface and the heights of land, with but little stratification."[24] There can be no doubt that this formation is the exact equivalent of the member number three of this paper, nor that there is a like equivalence between the laminated clays of Whittlesey and the formation here designated member number four.

Dr. J. S. Newberry found the equivalent of Whittlesey's laminated clays divisible into two parts in Ohio. "Of these, the first

[20] *Op. cit.*, p. 463. A similar succession was observed at Oshkosh, Wis.

[21] "Fresh-Water Glacial Drift of the Northwestern States," *Smithsonian Contributions to Knowledge*, No. 197, p. 2.

[22] It must not be supposed from the last clause in this description that the formation is confined to water-courses, but only that it is not found overlying the outcrops of sedimentary and igneous rocks on the divides. In the profile accompanying this excellent memoir these clays are represented with scarcely a break from Lake Erie to Lake of the Woods.

[23] *Ibid.*, p. 10.

[24] *Ibid.*, p. 2.

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and lowest, though not always present, is a tough, blue, unstratified clay, generally thickly set with small stones; more rarely containing those of larger size, ground and scratched." The other is a "fine, laminated clay, without pebbles." "Both the laminated and pebbly clays are * * regarded as parts of one formation, and the name *Erie clay* is retained for that, since it was coined by Sir William Logan to designate its exact equivalent on the north shore of Lake Erie."[25] Dr. Newberry found the Erie clay resting on the older rocks, and does not note the infraposition of the stratum of sand and gravel described by Whittlesey — the number five of this memoir. Neither does he mention the finding of organic remains *in* the blue clay (indeed he distinctly states that they have not been found there (p. 32), though they have been reported as having been found within it — p. 26), his "Forest Bed" overlying that formation and being by him considered distinct from it. The deposit resting upon the forest bed is by this veteran geologist supposed to be of lacustral origin, though it abounds in *kames* and *eskers*, and is not lacking in bowlders of the northern rocks, granite, greenstone, mica, slate, etc., the presence of all of which is attributed to the agency of icebergs, during an era of continental submergence. In general terms his conclusion is — and that it is supported by facts, Dr. Newberry's long experience and well-known conservatism are a sufficient guarantee — that the forest bed rests upon true glacial drift, more or less modified, and is overlaid by what is not unmodified drift. The forest bed itself is described as "a layer of carbonaceous matter, with logs and stumps, and sometimes upright trees," with occasional beds of peat, and containing remains of *Mastodon*, *Elephas*, *Castoroides ohioensis*, *Bison latifrons*, and their contemporaries.[26] That the Erie clay is the equivalent of our member number four is indisputable; and that the vegetation of the Ohio forest bed was contemporaneous with that found in Iowa wells, is equally certain, as Dr. Newberry himself concludes.

The State Geologist of Indiana, Dr. E. T. Cox, describes the finding of "muck beds," with logs, small fragments of wood, bark, twigs, roots, peat, grapevines and shrubs, "the fruit of the buckeye, beech, hickory, and buttonwood," as well as remains of the elephant, mastodon, peccary, giant beaver, bear, and sloth, and a

[25] "Geological Survey of Ohio," *loc. cit.*, p. 3.
[26] *Ibid.*, pp. 3–4.

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bone which "resembles that of the Irish elk," all, as in Ohio, above the Erie clay or lower till (except in one instance), in what he considers to be a lacustral deposit. The stratum containing the remains is reported from the counties of Dubois,[27] Dearborn, Ohio, and Switzerland,[28] Warren, Lawrence, and Knox,[29] Brown,[30] Jennings, Ripley, and Montgomery.[31] In Dubois county the shrubs and grapevines were of "enormous growth," indicative of the "luxuriance of a warmer clime;" in Warren county the "roots of trees, shrubs and plants," are spoken of as "of pre-glacial age," being covered with drift (though it seems to have been, as is usually the case here, a part of the Erie clay, nothing else being ever spoken of as "glacial drift"); and in Jennings county the wood "had been very much crushed and twisted, showing that it had been subjected to very great force." The wood was so little metamorphosed here, however, as to be used as fuel, half a cord or more being burned. These reports show that in Indiana the forest bed (there can be no doubt that in some of these cases, if not in all, the wood is of the same age as in the Ohio forest bed) is sometimes distinctly above the Erie clay or lower till, as is usually the case in Ohio, and sometimes intercalated with its strata —*when it exhibits stratification.* It is subjacent to rearranged drift materials, as in Ohio, when at the top of the Erie clay, and never, so far as recorded, covered by unmodified drift, as in the localities examined by Whittlesey and in that described in this paper.

In Nebraska, a section on Oak creek, Lancaster county, where the surface is covered by the loess, showed the following sequence: —

| | |
|---|---|
| Loess, | 15 feet. |
| Drift, | 2 " |
| Peat, | 2 " |
| "Clay and black soil," | unrecorded. |
| Drift.[32] | |

The forest bed is common to many parts of Nebraska. It is "found in the glacial drift, and separates it into two portions."[33]

[27] "Geological Survey of Indiana," Ann. Rep. 1872, p. 193.
[28] *Ibid.*, p. 402, *et seq.*
[29] *Ibid.*, 1873, pp. 194, 270, 318.
[30] *Ibid.*, 1874, p. 83.
[31] *Ibid.*, 1875, pp. 172–173, 196, 393.
[32] "Superficial Deposits of Nebraska" (second ed. — from Hayden's Report for 1874), p. 5.
[33] *Ibid.*, p. 11.

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The Assistant State Geologist of New Hampshire, Prof. Warren Upham, mentions the division of the till into two members, by Otto Torell, of Sweden, "the lower characterized by its blue color, its compactness and hardness and its glaciated stones; the upper being marked by a yellow and reddish color, comparative looseness of the mass, and its angular or unworn bowlders;" and he adds that the same division is found in New Hampshire, where, however, the lower till is dark grey in color, and is distinctly laminated.[34] The occurrence of organic remains in any part of the drift is not recorded in this connection.

Mr. James Geikie, the Scottish geologist, gives a more detailed description of the Scandinavian drift in his "Great Ice Age." "It consists of two distinct layers, the lower of which is generally darker in color, and containing fewer big stones than the upper."[35] Between the two is found the equivalent of the Ohio forest bed, or at least a stratum corresponding with it, from which five species of shells and two of plants have been recognized, and which is supposed by the Swedish geologists to represent a warmer inter-glacial era. The same author tells us that in Switzerland the lowest glacial deposit is "a dark tough clay packed with scratched and well-rubbed stones, and containing here and there some admixture of sand, and irregular beds and patches of earthy gravel."[36] It is quite unstratified, if we except these occasional intercalated gravel-beds. Overlying this is found sometimes, as at Dürnten and Wetzikon, in the canton of Zurich, the homologue of the forest bed here taking the form of beds of lignite from two to twelve feet in thickness.[37] Numerous remains of trees, identical with or related to the species now inhabiting the same region, are found, from an examination of which, Prof. Heer (than whom there is no higher authority) concludes that the climate of Switzerland during the inter-glacial period was not materially different from now. Associated with these have been found the bones of the Asiatic elephant, a species of rhinoceros, the urus, the great stag, and the cave bear, which affords evidence that the deposit of that formation was synchronous with that of our own forest bed. Above this are found "a considerable thickness of gravel and sand, in beds which are surmounted with several large alpine erratics."

[34] *Am. Jour. Sci.*, III, Vol. XIV, Dec., 1877, p. 462.
[35] *Op. cit.*, p. 354, *et seq.*
[36] *Loc. cit.*, p. 373.
[37] *Loc. cit.*, p. 375.

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"A similar succession of deposits has been detected by Professor Hanns Höfer, as occurring in Carinthia," where the organic stratum yields remains of the woolly rhinoceros, the steinbock, and *Bos taurus*.[38] To these instances of inter-glacial beds Mr. James Croll, the author of "Climate and Time," adds others at Dranse (Switzerland), Hoxne, Chapelhall, Craiglockhart, Lieth-walk, Redhall Quarry, Beith, Crofthead, and Kilmaurs in Great Britain, with others at many localities.[39] In every such case the organic stratum is found between beds of true glacial drift, often unmodified. Indeed, the presence in Great Britain of a lower till usually stained with protoxide of iron, resting on the older rocks with or without the infraposition of a thin gravelly or sandy stratum, and overlaid by first, a deposit containing organic remains in abundance, and second, a true glacial drift, has long been known; and descriptions of the phenomena by Croll, Geikie, Belt and others, are within the reach of all—though Mr. Belt's ideas, relative to the deposit of the materials, differ from those of the first-named geologists in important particulars.

These references will be sufficient to show that not only the formation here described as member number four, but the general succession of deposits in northeastern Iowa, are not peculiar to a limited area, but are co-extensive with glacial indications.

As has been seen, there is some variation in the position of the forest bed or carbonaceous stratum, with relation to the bowlder clay or laminated till, causing an apparent discrepancy between sections taken at different localities. While it is found in Ohio to overlie the Erie clay and be clearly separable from it, no such distinction is observable in the counties here described. The inter-glacial formations of both insular and continental Europe are also in most cases quite distinct from both tills, though Mr. Geikie speaks of the reported finding of "bones" and "horns" in the lower member.[40] The finding by Whittlesey, also, of the bones of *Elephas primigenius*, with sticks and leaves, clearly *above* the blue clay at Cleveland,[41] and in other instances *in* the same member (*when it was distinctly stratified*—see sections pp. 12, 13), would seem to indicate that the proper stratigraphical position of

[38] *Loc. cit.*, p. 377.
[39] *Op. cit.*, p. 239, *et seq.*
[40] "Great Ice Age," p. 183.
[41] "Glacial Drift," etc., *loc., cit.*, p. 8.

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the forest bed is above the lower drift, but that in some cases the materials have been re-assorted by aqueous action and the organic remains submerged below or elevated above their true place. Hence, in northeastern Iowa where the lower till is thin, it has been generally so thoroughly modified and worked over, that wood may be found in any part of it or even in the gravel below it. The position of the carbonaceous stratum varies so much that I have not assigned any specific division to it in my sections. It is also noteworthy that when the lower till is stony, it is not laminated or clearly stratified, nor does it rest upon a pebbly stratum — the member number five of this paper. Thus it is in such cases identical with the unmodified bowlder clay of other localities. This would indicate that this deposit may or may not be modified — a conclusion already arrived at indeed by Dr. Newberry, induced by the laminated and pebbly character of the upper and lower members respectively of the Erie clay in Ohio. The occurrence of this member in Ohio, Indiana, Missouri, and Kentucky, without the supraposition of member number three, as in Iowa, Wisconsin, and northern Illinois, would also indicate that its range southward is greater.

The origin of the five members described, beginning with the lowest, may be provisionally attributed to the following agencies: —[42]

An unbroken ice-sheet extended over the greater part of the northern hemisphere north of about latitude 40° N., and in its southerly motion ground the surfaces of the older rocks and reduced their upper strata to bowlders, pebbles, coarse gravel, fine gravel, sand, and impalpable powder. These were deposited below its base in depressions of the surface, and at its margin as it retreated northward. Where there were aqueous currents these materials were deposited to some extent in the order of their fineness — the coarser and heavier near the bottom with but little arrangement, and the lighter near the top, where, being subjected to the action of the glacial floods, they were still further re-

[42] That any explanation of drift phenomena can only be provisional is obvious, when we remember that there is the widest difference of opinion among competent geologists as to the agencies producing *kames*, glacial striæ, and even the drift itself; and as that here offered introduces a new element, it must, *a fortiori*, be looked upon as liable to require modification or abandonment, though it seems to accord fully with the facts as observed.

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arranged. Thus we have at the base of the glacial deposits a coarse pebbly clay containing bowlders (member number five) overlaid by a fine clay frequently laminated and stratified (member number four). Where the total thickness of this glacial deposit is great, as in Ohio, we find a thick stratum of bowlder clay overlaid by an equally thick stratum of finely comminuted and horizontally laminated materials. Where there were no powerful aqueous currents accompanying the grinding up of the rocks, the materials were laid down indiscriminately mixed, and with little or no stratification, forming the typical bowlder clay. During the final melting and disappearance of the glacier, when the greater part of the surface was acted upon by powerful currents of water, further assortment and stratification of the materials probably took place, together with the formation in some places of *kames* and *eskers*. This will explain the irregularities in surface and stratification sometimes exhibited in the blue clay, as attested by the sections of Whittlesey, Newberry, and Geikie.[43] Previous to this glacier, outcrops of hornblendic rocks, syenite, diorite, and porphyry, probably existed associated with or overlying the granitic strata of Minnesota, the material from which was all carried away and distributed over the surface far to the southward, where it now occurs as bowlders and pebbles, nearly always much worn and sometimes striated. After the final retreat of the glacier, the rivers and streamlets eroded their valleys afresh, and the topography assumed nearly its previous aspect.

Next ensued a period of mild climatal conditions which must have been of immense duration, during which a luxuriant *flora* covered the surface, and a characteristic *fauna* was developed. Where the surface deposits were thin, they were almost wholly metamorphosed by the reconversion of their elements due to the action of vegetation, and a layer of vegetable *humus*, many times as thick as that now existing was spread over the surface. Indeed it is probable that the whole or nearly the whole of the underlying drift was modified in color at least by the vegetable accumulations above. As before stated, the lower till owes its blue color to the presence of protoxide of iron. Now it was long ago shown by Dr. T. Sterry Hunt that the presence of decomposing vegetable matter is requisite to extract the iron from earth and rocks, after which it

[43] Whittlesey, *loc. cit.*, p. 12, *et seq.* Newberry, *loc. cit.*, pp. 24, 35–36. Geikie. *loc. cit.* pp. 138–140, 164, 169.



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is deposited as an oxide. The bog-ore found in the forest bed undoubtedly owes its origin to such agencies; and it is highly probable that the prevailing color of the till is due to the same causes. Beds of peat were formed abundantly and of great thickness; and the mammals whose remains have been preserved were of elephantine proportions. These facts, with the almost tropical luxuriance of the forests, attest a mild climate and favorable conditions to the production of vegetable and animal life. The close of this vast non-glacial period was characterized by the presence in great numbers of individuals representing the species *Mastodon Americanus*, *Elephas primigenius*, *Bison latifrons*, *Castoroides ohioensis*, *Equus major*, *Bootherium cavifrons*, *Dicotyles compressus*, and their contemporaries, and of a great profusion of forest trees quite similar to those forming the forests of to-day. These, with the vegetable *humus* and accompanying peat-beds, constitute the American forest bed. In Europe were formed synchronously the peat-beds and buried forests of Switzerland, Scandinavia, and Great Britain; and the dense forests were the resort of the hairy mammoth, the woolly rhinoceros, the urus, the great Irish stag, the cave bear, and paleolithic man.[44]

The last glacier then advanced, and in its progress removed from many localities the accumulations of the previous period, and modified them to a greater or less extent in all places. Sometimes the weight and impetus of the mass contorted and pressed out of place the strata of the lower till, as in the section shown in figure 2 and in the analogous sections cited from Geikie, without entirely removing them. From the surface of this member, as well as from the exposed outcrops of sedimentary rocks (and it is not impossible that the great pressure and low temperature to which the earthy materials were subjected, would render them more resistant to the action of the glacier than even the harder rocks) a mass of detritus was gathered and carried along by the glacier; and the accompanying floods re-arranged still further the lower till, sometimes mixing slightly the materials of

[44] There can no longer be any doubt as regards the contemporaneity of man and these extinct mammals in Europe. Some decisive cases are recorded by Dr. R. H. Tieddeman in *Nature* for Oct. 5th, 1877. There are still doubts as to the pre-glacial age of man on this side of the Atlantic, notwithstanding the seemingly conclusive evidence afforded notably by the finding of rough stone implements in the drift of New Jersey, by Dr. C. C. Abbott, and of a human cranium in the auriferous gravel of Table Mountain, California, by Mr. C. F. Winslow. Further evidence to the same effect is also on record.

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both. The fragments of wood, and the accompanying teeth and bones were sometimes shifted in position, and placed above or below their proper place at or near the surface of the lower till. As before, bowlders of northern rocks were brought down and scattered over the surface and through the later drift; but where the lower till was not removed these bowlders would not exhibit striæ. Neither would striated surfaces be found in such localities, unless they belonged to the epoch of the lower till—an epoch too remote to permit of their preservation. *Kames* and *eskers* were formed by the action of the superficial rivers occasioned by the melting of the surface of the ice-sheet, and ultra-glacial rivers assorted and re-arranged the materials near the surface; and the unmodified and slightly modified drift was formed. This glacier did not extend as far southward as the last, leaving a belt of lower till uncovered, or buried only beneath the finer materials borne towards the Gulf by broad and sluggish rivers. Here the surface accumulations of the preceding warmer period were simply buried in their place, and retained their true position relative to the lower till; but the fine alluvial material covering them is not a true glacial drift, such as is found farther to the northward. Rivers from the melting ice-sheet eroded the surface afresh as the temperature increased, and once more the topography assumed approximately its pre-glacial aspect. Member number three now formed the surface.

Again a slight advance of the glacier took place, and the strong current of its rivers transported a quantity of detritus that the slower currents of the larger streams, which had but little fall, could not carry away, and their channels became silted up. Basins were thus formed along and near rivers having not a rapid descent, and after a slight disturbance of the surface, finely comminuted materials held in solution by the waters, gradually became separated and sank to the bottom. During the first increased action of the rivers the finer *debris* was carried away and the coarser remained; and it forms the member number two of this paper. Then the finer materials were spread over the same surfaces as they dropped from the waters filling the basins, forming the last of the surface deposits—the member number one of this memoir. During and after the final retreat of the glacier erosion progressed, and the surface gradually assumed its present outline.

A sufficient mass of facts bearing on the character of the forest

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bed with its *fauna* and *flora* has been accumulated through the efforts of various observers, to provisionally determine the true place of the epoch which they represent in the geological scale.

It must be premised that only during an immensely long period could so vast an accumulation of vegetable matter as that forming the forest bed have taken place; and we have the authority of Dr. Newberry and other distinguished geologists for the statement, that the forest bed proper is found *only above* the lower till. As we have seen this is universal except in cases where there are the best of reasons for inferring that re-arrangement has occurred since the formation of that stratum. Hence, not only the development of an exceedingly rich and luxuriant *flora* and its extension over the barren plains of a freshly glaciated hemisphere, but the development—including without doubt the evolution from the lower forms of a preceding eon—of a characteristic *fauna*, must have taken place within this period. And it is probable that the same period embraces more nearly the whole of the mastodon epoch than is generally supposed. Prof. Hall has shown[45] that both the Cohoes and Warren (Newberg) mastodons must have been of pre-glacial or inter-glacial age, as well as the remains found at Big Bone Lick, in Kentucky,—in fact he mentions two specimens of mastodon tusk from the latter locality, which clearly exhibit glacial markings, similar to those of drift bowlders. It is a noticeable fact, also, that skeletons of these animals when at all perfect, or in which the members occupy their natural positions, are almost invariably found in swamps or morasses in which it is usually supposed the creatures were mired and suffocated,—the improbability of which is insisted on by Prof. Hall. Now the lower till is frequently almost impervious to water, as has already been mentioned, and when the later deposits have been removed by denudation, a swampy depression may be formed, with small springs seeping into it from its margins. Such morasses are not at all uncommon. Should a mastodon skeleton be found in such a slough, especially if surrounded by peaty matter with mouldering fragments of wood —and this is the usual environment when mastodon remains are found—it would in nine cases out of ten be recorded as of post-glacial age, even though associated directly with inter-glacial *debris* and entombed within the lower till. And if the locality were a valley and the remains had been removed and the materials

[45] "21st Annual Report of the New York State Cabinet," p. 103, *et seq.*

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surrounding it assorted by moving water, the "find" would be credited to the alluvion, and also considered recent. Such misapprehensions have undoubtedly occurred. While, therefore, it could not be postulated that the mastodon became extinct before or during the last glacier, we may assume with Prof. Hall that its remains have been rarely if ever deposited under such conditions as to be preserved, since that cataclysm. The presence of unique genera (as compared with those of earlier time) suggests a long period of development. Favorable climatal and other conditions during many generations are requisite, according to the principles of natural selection, to form and render permanent the wider differences in any class, while specific differences may be due to sudden alterations in environment not great enough to work immediate destruction and extinction. Liebig estimated that the time required for the formation of the peat-beds at Dürnten, Utznach, and Mörschwyl, Switzerland, now transformed into lignite, must have been 9600 years.[46] If reasonable allowance is made for the time employed in the retreat of the glacier and for the acclimatization and spread of the inter-glacial vegetation, it will be seen that one of the inter-glacial periods of Croll would be too short to permit of such an accumulation of vegetation. These considerations strengthen the conviction that the forest bed epoch was one of immense duration.

There cannot be the slightest doubt that the greater part of the land surface of the northern hemisphere occupied nearly its present position and preserved nearly its present level with relation to the sea, and there is the strongest *a priori* probability that it was the habitat of a terrestrial *fauna* and *flora*, during the Pliocene period; and the consideration of the palæontological relations of both the plants and vertebrated animals of the forest bed, shows that there is a close affinity between them and those of the Pliocene. The provisional assumption, therefore, that the forest bed represents the close of the Pliocene period, need not be considered wholly unwarrantable. The earlier part of this period is undoubtedly represented by the *Loup River Group* of Prof. Hayden, which contains fossil remains of camels, elephants, rhinoceroses, horses, etc. The Loup River formations are unquestionably of lacustral origin, and may have been deposited shortly after the retreat of a glacier, just as the loess of Dakota, Ne-

[46] *Vide* "Climate and Time," p. 241.

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braska, and western Iowa was laid down in nearly the same region at the close of the later glacier. Thus a glacier may have been the cataclysm that separated the Miocene from the Pliocene, as well as the Pliocene from the present—the first of the two forming the lower till, and the second the upper drift. The intercalated fossiliferous stratum found in the drift throughout the northern hemisphere may, therefore, be the exact equivalent of the marine Pliocene beds deposited where the surface was submerged.

As has already been intimated, Croll considers the fossiliferous beds intercalated within the drift of Great Britain, to represent only those comparatively brief warm periods occurring during any era of great eccentricity of the earth's orbit and consequent glaciation of the two hemispheres alternately as the revolution of the apsides progresses. Such warm periods would occur to either hemisphere when its pole was directed towards the sun while the earth was in aphelion. But, as just suggested, such a warm period would be too brief to answer the requirements of the forest bed epoch; though during these the thinner carbonaceous deposits above the "Cromer Forest Bed" and the forest bed proper at some other localities may have been formed. Croll's interpretation of geological and physical facts also causes a break in geological history, the Pliocene period being necessarily considered either to have been deficient in terrestrial life, or to have had all traces of its fauna completely obliterated. Obviously, however, Croll's determination of the times of the last glaciers is equally applicable if the sequence here suggested is correct. This geologist and physicist has shown in "Climate and Time" (partly through the use of Mr. Stockwell's calculations) that periods of great eccentricity of the terrestrial orbit occurred 210,000 and 850,000 years ago respectively; and the complicated chain of physical agencies by which continental glaciation is effected during such periods, is unravelled and the glacier-forming processes elucidated in the same connection. The most recent of these he believes (in opposition to the Lyellian school, according to which the ice-age was much more remote, Sir Charles himself having been inclined to believe that the most recent glaciation took place concurrently with the earlier eccentricity) to have occasioned the last glacier, and the more remote of the two, a glacier of the Miocene. It is not improbable, however, that this last occurred at the end of the Miocene, and, closing that epoch, inaugurated the Pliocene.

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This interpretation will allow of sufficient time to explain all phenomena of the Pliocene and of the probably equivalent forest bed epoch; and it harmonizes perfectly with the ablest effort of recent years to establish a correspondence between the geological record and the physical history of our planet. One striking coincidence—if nothing more—is observable between Croll's theories and the historic sequence here advocated. During the maximum eccentricity of 850,000 ago the glacial conditions were more favorable than at any time during the last era of great eccentricity. In the first period the excess of winter over summer in days in the glaciated hemisphere was 34·7; while in the most recent the excess was only 26·7—a difference of eight days, or more than 33 per cent. Hence, according to theory, the ice-sheet must have reached much nearer to the equator during the first of these periods than during the last. Now as imperfectly shown in this paper and more fully in the works cited, the drift supposed to be of the anterior glacier extends many miles farther southward than that attributed to the recent glacier; and it is to this more southerly distribution of the lower till than the superincumbent drift that the apparent confusion in regard to the stratigraphical position of the forest bed is to be ascribed.

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## Richthofen's Theory of the Loess, in the Light of the Deposits of the Missouri. By J. E. Todd, of Tabor, Iowa.

In reading the descriptions of the Chinese Loess, derived from Baron von Richthofen's great work, one in western Iowa wonders at the close resemblances to his own region. The same language might be used to describe it, word for word, sentence for sentence, except that a few diminutives must be thrown in when altitudes are given.

Of the *surface* features, we have his cañons and chasms in our ravines and "break-offs." Instead of "hundreds of feet" in depth, they are rarely over forty feet deep. They present the same phenomena of erosion. We sometimes see the "rock of

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earth steeper than any rock of stone," surmounted with solid structures which do not find their confidence misplaced.

Taking the whole Missouri valley, we have the formation at widely varying altitudes, from about 3000 feet on the North Platte down to the level of the sea.

We have the same rapid erosion by surface waters and winds. In early years Indian trails rapidly grew into gullies, and cases may be shown where roads have sunk from such cause six feet in a dozen years. Given the time, and the depth of the Chinese Loess, and we might match the Chinese roads which he mentions, as having sunk in gorges 100 feet deep.

It should be stated here that, owing to the relative shallowness of the Missouri Loess, the ravines and cañons which were probably a characteristic feature centuries ago, have dwindled from their former size, since the principal streams have reached the firmer underlying formations. In China they still form the prominent feature.

Looking more closely at the *structure* of the deposit, we find the same minute vertical tubes inosculating at acute angles, to which he calls attention in the Chinese Loess. They are likewise frequently lined or filled with calcareous matter.

The Loess of western Iowa presents the same common absence of stratification, and has the same vertical cleavage and underdrainage. It has the same lines of calcareous concretions, which lines are usually horizontal. The concretions, moreover, are often elongated, with their longer dimension vertical.

We have the same yellow earth, and the *Missouri*, or "Mud River," is very closely the counterpart in color, of its cousin, the *Hoang-Ho*, or "Yellow River."

It seems, therefore, not unreasonable to interpret and test von Richthofen's deductions from the Loess of China, by the light of the deposits of the Missouri valley. Certainly we may do this, when he extends his theory to this very region itself, in common with others.

Von Richthofen's theory, as we learn it from the various reviews of his work, is briefly this: that *loess*, certainly in China and probably in all continents, is a *sub-aërial* deposit collected on dry grassy areas by the action of fierce winds. For the formation of such a region, he supposes a central undrained elevated area, from which nearly all moisture is excluded by surrounding mountain

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chains. The central plateaus of Asia are, at present, the nearest approach to his ideal.

His novel conclusion that it is of sub-aërial origin is based upon four things: —

1. The constant occurrence, throughout the formation, of delicate elongated cavities, such as are formed by the rootlets of plants.

2. The occurrence of innumerable land-shells, and bones of land animals, but no traces of fresh water or marine life.

3. The entire absence of stratification.

4. The occurrence of the deposit at very widely varying altitudes.

Let us consider these points in reverse order, taking up first the least questioned and least significant.

I. The occurrence of the Loess, at widely varying altitudes, is as true in America as Asia, the limits corresponding closely with the average altitudes of the respective continents. This, however, is no more than can be said of any unquestionably aqueous formation, like the Potsdam or Cretaceous. If reference is made, however, to a very peculiar character of the Loess, viz., its approximately uniform depth on hill-top or hill-side, this is quite as explicable by a sub-aqueous theory as by a sub-aërial. The Loess in southwestern Iowa and Nebraska overlies older formations very much like a great blanket. This has led to frequent over-estimates of its thickness. The vertical distance from its lowest point in the sides of a valley to the top of an adjacent hill, for example, is far from being the true thickness, because the Drift, and even the Carboniferous sometimes rises up within a few feet of the top of the hill. This is supported by numerous observations in railroad cuts and wells. It is shown, therefore, that the same general configuration of surface has been preserved in this region from the close of the Glacial Period, or throughout the Loess epoch.

This is easily explained by the usually received sub-aqueous theory, by supposing that the silt was deposited with some degree of uniformity, over the whole bottom of old Lake Missouri; that, before the inequalities of its bottom had been smoothed over by complete filling to the water level, the lake was drained, and streams followed the same channels as before, except that they were two or three hundred feet higher.

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If, on the other hand, the deposit had accumulated by the action of winds, it is not only a little difficult to see how there could have been any considerable deposition upon the hill-tops of the Drift, but more difficult to see why it would not have accumulated so rapidly in the lee of higher elevations and so filled up valleys in such positions that streams could not have generally found their original courses.

Before leaving this point it is interesting to note that the Baron, though assuming that relative elevations were not much changed during the Post-pleiocene, yet to account for the recent drainage of his central areas, supposes a depression of the seaward side, which is the same and only change in relative elevation which the sub-aqueous theory postulates. His theory assumes, however, a greater absolute elevation as before stated. This furnishes an objection very serious in the case of China, while with American deposits it is apparently fatal to his theory. We fail, therefore, to see how widely varying altitudes, whichever way we interpret the expression, favor the sub-aërial theory.

II. Of the absence of stratification in the Loess, which is the uniform statement of observers, using terms as they are applied to older formations, little need be said.

Surely, the deposit is "massive" rather than "stratified," but nevertheless, to close observers, as has been frequently stated, it shows quite generally a *lamination* indicating its aqueous origin. Moreover, the differences in color and hardness, as is sometimes brought out by sub-aërial erosion, frequently over considerable areas, show a stratification so closely resembling that of ordinary sub-aqueous deposits, that, to say the least, the burden of proof lies on the other side. We simply say that either the Loess of China differs from our own in this respect, or the Baron does not use terms very exactly.

III. The Loess of the Missouri does not differ from that of other regions in the general character of its fossils. Land shells greatly predominate, purely aquatic species are rare, the more so I think if careful discrimination is made between the true Loess and the modified. However, some semi-aquatic species, as *Succineas* and *Helicinas* are very abundant from top to bottom of the formation, and the decidedly aquatic *Limnea humilis* is quite abundant in the upper Loess of western Iowa. This is sufficient

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to show that the formation, so far as this region is concerned, could not have been so dry as is called for by Richthofen's theory. Even the land-shells observed, need a much moister region than our upland prairies in their present conditions. Some of them are not found at all, away from moist banks or groves.

The obvious paucity of fresh water life, which seems at first view mysterious and perhaps irreconcilable with the usually received sub-aqueous theory, may be perfectly explained when we consider the vast size of the lake or lakes—its waters cold and supporting little vegetation, and its shores nearly destitute of rocks or pebbles, which seem almost essential to many fresh water mollusks.

The small size of the *Limneas* and absence of *Physas*, in the latitude of Iowa, may be considered as indicating that the waters were cold, while the occurrence of numerous land-shells of species still inhabiting the region, indicates that the lands were more moist, with their temperature not differing greatly from the present. I think no one can study the life of the Missouri Loess without seeing that it bears, throughout, evidence of a moist climate, the very opposite of the Baron's ideal.

FOSSILS FOUND IN THE LOESS OF S. W. IOWA.

*Hyalina Binneyana* Morse, common.
" *fulva* Draparnaud, common.
" *lineata* Say, common.
*Helix alternata* Say, common.
" *perspectiva* Say, rare.
" *striatella* Anthony, very common.
" *labarynthica* Say, very rare.
" *monodon* Rackett, very common.
" var. *Leaii*, very common.
" *divesta* Gould, rare.
" *multilineata* Say, common.
*Cionella subcylindrica* L., common.
*Pupa muscorum* L., rare.
" *Blandii* Morse, rare.
" *fallax* Say, rare.
" *armifera* Say, very common.
*Vertigo Gouldii* Binney, common.
*Succinea ovalis* var. *De Campii?* Tryon, common.
" *lineata* Binn., common.
" *avara* Say, common.
" —— var., common.
" *obliqua* Say, common.
" var. *Greerii* Tryon, very common.
*Helicina occulta* Say, very common.
*Lymnea humilis* var. *parva*, common: upper.
*Pomatiopsis lapidaria* Say } probably from Modified Loess.
*Planorbis trivolvis* Say }

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PROF. S. AUGHEY
REPORTS NONE OF THE ABOVE DEFINITELY, BUT REPORTS THE FOLLOWING FROM THE MIDDLE LOESS OF NEBRASKA.

*Helix Cumberlandiana* Lea.
" *Hubbardi* Brown.
" *auriformis* Bland.
" *tholus*? Binn.
" *fastigans* Say.
" *Jacksonii*? Bland.
" *Hazardi*? Bland.
" *Dorfeuilliana* Lea.
" *pustula*? Fer.
" *spinosa* Lea.
" *Edgariana* Lea.
" *Roemeri* Pfeif.
*Bulimulus dealbatus* Say.

PROF. G. C. SWALLOW
REPORTS FROM LOESS OF MISSOURI.

| | | | | | |
|---|---|---|---|---|---|
| *Helix*, | 17 | species. | *Lymnea*, | 8 | species. |
| *Physa*, | 8 | " | *Planorbis*, | 4 | " |
| *Pupa*, | 3 | " | *Succinea*, | 6 | " |
| *Vulvata*, | 1 | " | *Amnicola*, | 1 | " |
| *Helicina*, | 1 | " | *Cyclas*, | 1 | " |

IV. With reference to the occurrence of root-marks, unusual care is necessary to interpret observations correctly. A series of observations might be easily made which would seem to show that they are found with great uniformity from top to bottom of the formation. They may be found at the base of the formation apparently too far below the natural surface to be connected with recent plants. Plants grow so readily upon recent exposures of the deposit that marks may be found in very deep and recent excavations, and attributed to very ancient plants, which have been formed within the last five years. Guarding carefully against such mistakes, a careful examination of the Loess of the Missouri shows the following limitations to the constant occurrence of said root-marks. Observations have been greatly assisted during this season by the freshets providing many fresh vertical exposures in favorable localities.

1. Root-marks, as a rule roughly stated, may be said to vary in frequency inversely as their distance below the present surface. Near the surface besides being most abundant, larger ones are found. At the depth of thirty or forty feet they are very minute and rare. None have been found lower than about forty-five feet, although several favorable localities have been examined.

[1] Report of the U. S. Geological Survey of the Territories, for 1874.

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2. The lower limit of root-marks is approximately parallel with the present surface. This is especially well shown in the deep cut through a bluff at Brownville, Nebraska.

3. There are, moreover, certain bare cliffs of Loess in which no root-marks are found, at least not near the front surface, except in crevices. Apparently, the present surface of the cliff was shaped before vegetation began to grow upon it, and the elevated dry position renders the front of the cliff too hard for roots to penetrate.

4. Certain other portions of the Loess have also been observed at different depths, which have for some reason resisted the penetration of roots. They sometimes occur in extensive beds and near the surface, as may be seen in the bluffs near Council Bluffs, Iowa.

These observations suggest and force the impression upon us that these root-marks are all connected with plants growing at or near the present surface; provided we can believe that the roots of herbaceous plants—and we find no trace of tree roots on the prairies—ever go to the depth of forty feet. The surface has changed much since the present conditions have prevailed. In some cases the carbonized soil shows where a hollow has been filled twenty feet or more, while at other points the surface has been rapidly lowering. But these marks are found forty feet deep, where there is no such evidence of the surface rising.

The whole force of the Baron's argument on this point is based upon the very plausible assumption that these root-marks could not have been formed by plants growing at the present surface. That, therefore, the surface must have risen while the plants were growing, or in short that the formation must have been sub-aërial. It readily appears that this point first suggested the sub-aërial theory to the Baron's mind. It becomes, therefore, a most interesting problem to discover the depth to which the roots of living plants penetrate this formation. The following table presents a few of the most characteristic and authentic observations which have been made by the writer the present season.

| | | |
|---|---|---|
| All annuals and most perennials penetrate the Loess | from 1 to | 6 feet. |
| *Lygodesma juncea* Don | over | 8 " |
| *Poa pratensis* L., Blue-grass, | | 10 " |
| *Kuhnia eupatoroides* L. | over | 11 " |
| *Rhus glabra* L., Sumach, | " | 12 " |
| *Amorpha canescens* Nutt., Lead-plant, | " | 15 " |
| *Vernonia nova-boracensis* Willd., Iron-weed, | " | 17 " |
| *Spartina cynosuroides* Willd., Marsh-grass, | " | 20 " |
| Grass? undetermined, | " | 21 " |
| *Equisetum lævigatum* Braun., Scouring Rush, rootstocks | " | 23 " |
| *Equisetum lævigatum* Braun., Scouring Rush, roots | " | 25 " |

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In all cases recorded above the rootlets have been living. Dead ones, still showing the woody fibre, have been found at lower depths.

When we consider, therefore, that different plants habitually penetrate to greatly differing depths, and that further examination might discover some penetrating still deeper, or when we conceive of the effect which climate has upon this feature of vegetation, we find it comparatively easy to believe that the limit of forty-five feet, before mentioned, might be reached, either by plants now extinct in this region, or by the existing species under different climate. Observations in drier regions, or where the summers are drier, would be most interesting and instructive for comparison.[2]

The distribution of root-marks, therefore, when rightly interpreted, opposes rather than supports Richthofen's theory.

V. Other objections to the acceptance of this theory, at least for the Loess of the Missouri, other than those already mentioned or implied, may be briefly stated.

1. The close resemblance of the formation to the present alluvial deposits of the Missouri, both in chemical and physical characters, cannot be easily reconciled with a sub-aërial origin, of which we should expect more of the finer—clayey, carbonaceous and calcareous—elements, and less of the silicious.

2. The elevation of the seaward side of the Loess area for such an interminable age as would be necessary for accumulation by wind, and the degree of elevation sufficient to keep out moisture, is clearly irreconcilable with the geology of surrounding regions, which has been reasonably well established for the Post-pleiocene.

3. The inadequacy of the cause need not be dwelt upon. It is simply astounding, and only the more so, the more we attempt to supply the necessary adjuncts. We conclude, therefore, that Baron von Richthofen's theory utterly fails when applied to the Loess of the Missouri.

The features upon which it depends, although at first appearing promising, when carefully examined not only betray their insufficiency, but demand an opposite explanation.

The formation, though not marine, shows distinctly the action of water. The root-marks, instead of being really *throughout* the formation are very clearly referrible to plants growing at or near

[2] Prof. C. E. Bessey, of the Iowa Agricultural College, informs me that *Lupinus arboreus* is reported from California, as sending down its roots sixty to seventy feet, in the dry sand hills near the Golden Gate.

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the present surface. The fossils though mainly terrestrial are interspersed with amphibious and aquatic forms.

Moreover, with reference to the Loess of China we may be allowed to speak briefly. When we consider how the features which led the Baron to his conclusion, although apparently equally promising at first in the case of the American Loess, are when better seen found to face the other way; when we think of the vast depth of the Chinese Loess which renders the proposed explanation so much the more inadequate; when we reflect upon our comparative ignorance of Asiatic geology; when we remember Prof. Pumpelly in his quite extended observation of the region saw nothing inconsistent with a lacustral origin, we may express our confident expectation that ere long Richthofen's theory will be shown utterly inadequate to explain the formation which first suggested it. Even now we probably express the opinion of many students of the Loess, when we say that the sub-aërial theory has received attention and consideration, mainly because of the boldness and novelty of its conception, and the high rank and well-deserved reputation of its author, rather than on account of its real merits.

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## Ancient Glacial Action, Kelly's Island, Lake Erie. By Chas. Whittlesey, of Cleveland, Ohio.

In September, 1873, I visited Kelly's Island in Lake Erie, for the purpose of examining the great ice grooves for which it is remarkable, especially those at the quarries on the north side.

Kelly's Island is one of a group situated near the west end of the lake in shallow water, all of them of *limestone*, in nearly horizontal beds. None rise many feet above the water level.

They formed originally a part of the mainland on the south, and of the low coast to the west. Probably all of the lake, west of Point Pellee, in the pre-glacial period, was more land than water. Instead of a lake with islands, it must have been country with lakes, rivers and swamps.

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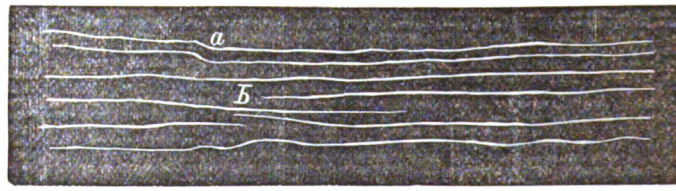
Wherever the rocks are laid bare, the evidences of ice action are universal and very marked. In Sandusky city many of the cellar bottoms show polished, grooved and striated surfaces of rock.

Around the islands, and along the shore, wherever the waves have carried away the drift hard pan covering the rocks, large spaces are laid bare, and the grooves and striæ are seen, as fresh as those beneath the present glaciers of the Alps.

A very convenient place to observe them is at the southeast corner of Kelly's Island, about a mile east of the *Island House*. Here the rock is very smooth and level, and the lines are remarkably straight. A sample of this glaciation embracing a length of 300 feet is here given.

Fig. 1.




Glacial lines, southeast corner of the Island, space represented 300 feet in length — general bearing S. 75° west — intermediate fine lines not shown.

The bearing of the straightest and best defined lines is S. 75° west, with slighter ones crossing these at N. 80° west, magnetic. There were a few straight grooves observed, bearing N. 25° west. These lines show a slow direct motion to the west, with short changes of direction, sometimes quite sudden, after which the original course was resumed, as at *a*, Fig. 1. In size they are from a mere line to a depth of an inch, with a variable breadth seldom greater than twice the depth. Such furrows are no doubt due to stones held firmly in the ice as it progressed. At *b*, a new groove commenced and another terminated. These stones are abundant in the hard pan, showing flat and striated facets, where they came in contact with the rock. Smaller lines and polished surfaces are due to the joint action of gravel, sand and ice.

This long-continued movement of many centuries' duration, along the axis of the lake from a northeasterly direction, cut away new channels between the islands, and ground down their surfaces. On their easterly sides the polished surfaces extend beneath the water. As the mainland rises from the coast towards the interior,

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they are found at all elevations, to 700 and 800 feet above the lake. The thickness of the ice mass, must have been at least 1500 feet. This slow but resistless motion, crushed and transported the soft shales of the lake basin, and materially wore away the harder sandstone and limestone strata, south and west of the lake.

By this means the face of the country was rendered less rugged than it had been in the pre-glacial period. The summits were rounded, and the valleys filled with the debris.

Across the north side of Kelly's Island is a limestone ridge, which came near being separated from the southerly part and left in the form of a long rocky islet.

The ice movement passed over it lengthwise, striking it at the northeast end, where the dock and quarries of the Calkins Lime Company now are. This projecting buttress was scarred, crushed, pulverized, and partially torn away.

The evidences of this are still distinct wherever the rock is laid bare. South of the ridge is a long depression parallel with it, which is largely due to the erosive power of the all-pervading glacier. It is now a rich valley in the form of a bay, but little above lake level, covered with vineyards. The island embraces less than 3000 acres of surface, of which this is the most valuable part for agricultural purposes.

On the sloping sides of the ridge, the rock is polished and grooved in the same manner as on the flat surfaces. Long troughs, straight, crooked and warped, are thus scalloped out, many feet wide and deep, extending hundreds of feet in length.

The results of this action around Quarry Point, I propose to illustrate with some detail.

*Mr. John Dean*, the company's agent at the quarry, has taken an interest in the bold ice lines, exposed as the work of quarrying progressed. By his care, with the advice of company, he preserved the most remarkable of them for a considerable period.

Everything was thus in fine condition for examination. It was also arranged that photographs should be taken at several points.

This was afterwards done with good taste and judgment, by *Mr. A. C. Platt*, an artist of Sandusky. A coup d'œil sketch of the shore and quarry is here given.



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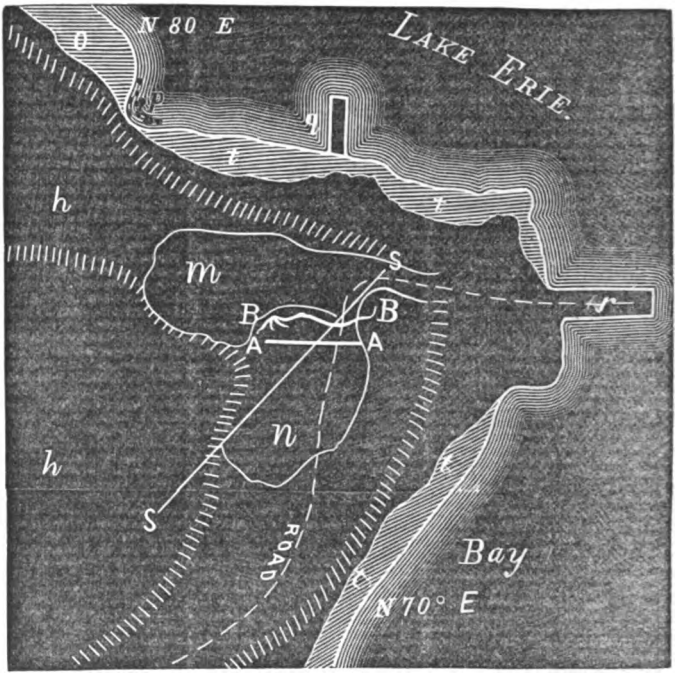
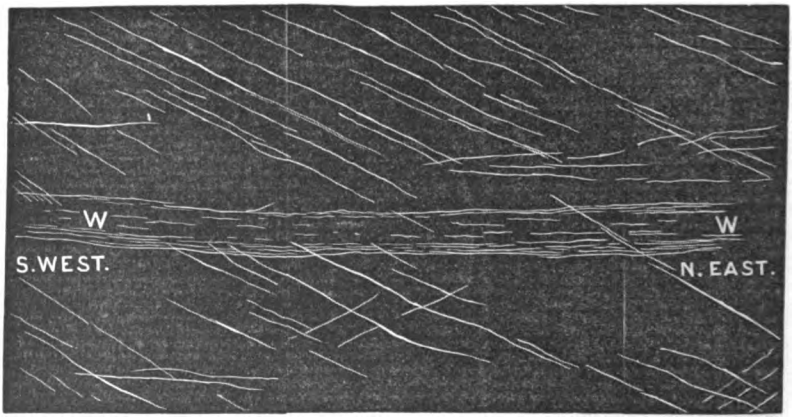
Fig. 2.




Plan of Quarry and Landing — scale 10 rods to the inch. *m*, quarry of 1873; *n*, old quarry; *r*, present dock and landing; *q*, Kelly's old dock, destroyed; *A*, large groove, straight; *B*, northerly groove, crooked; *p*, the angular stones of the old dock became pebbles by attrition; *t*, *t*, *t*, bare glaciated surface passing beneath the drift *h*; *o*, point where the tracing Fig. 3 was taken.

Fig. 3.




Glacial striæ at *o*, west of Kelly's dock *q*, bearing N. 80° west. The broad groove *W W*, N. 45° east. Scale one twenty-fourth of nature. Reduced photograph of a tracing on cloth.



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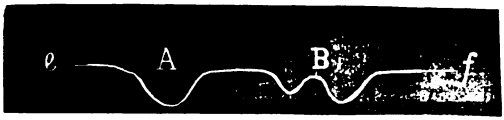
A few views and profiles are given to represent phenomena which are so rare in the history of glaciation that I have never seen another instance precisely like them. The cut (Fig. 3), representing the striæ on a large flat surface at *o*, is from a tracing upon muslin, taken by Mr. Dean and myself, size of nature, six feet by eleven, and photographed to one twenty-fourth of nature.

Profiles of the two largest and most singular grooves, *A* and *B*, of the coup d'œil sketch, are given in Figs. 4 and 5, much enlarged.

Fig. 4.

Profile on *c d*, Fig. 6, looking west. *A*, southerly groove; *B*, northerly groove.

Fig. 5.




Profile on *e f*, Fig. 6, looking west.

There is nothing unusual about the most southerly one *A A*, except its size. It is straight, broad and deep, and its axis is nearly east and west, parallel with the general course of the finer lines engraved everywhere on these rocks.

More interest attaches to the northerly one, *B B*. It is very tortuous and irregular, both in width and depth; and it is, therefore, quite difficult to convey a correct idea of its appearance.

The outlines of the horizontal plan, Fig. 6, represent the outer edges, where it is broadest. Mr. Platt's photograph, which is reproduced in Fig. 7, gives a lifelike view of the upper or west end, where it is descending rapidly towards the east.

On the hardest and least perishable part of the rock within the groove, there are very fine but distinct striæ, like those on the flat rocks adjacent, plainly showing ice action; but large portions of it are smooth, indicating that a part of the wearing process was due to water action. About sixty feet in length has been uncovered, of which something more than twenty feet was destroyed in working the old quarry. It can be traced across this excavation

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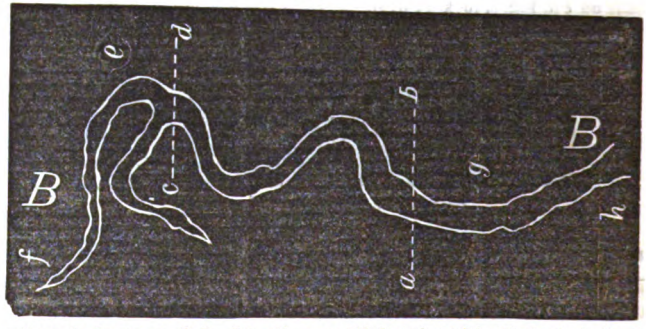
easterly, to a knob which is left standing a few feet high, where it is covered by drift. On the west, the prongs as they become smaller, are also filled up with loose drift materials *h h*. There are parts of it near the middle which are lower than either end, but there may have been in the old quarry other branches towards the east, which were lower and have been destroyed. One of these probably came out on the bay, near the most southerly part of the dock.

For the purpose of showing the changes of form which this crooked and fluted groove exhibits, I give the Figs. 4 and 5, cross-sections.

Fig. 5 is the first one on the east, and represents the wall *S S*, about twenty feet high, which bounds the old quarry *n* on the northwest. Here the trough *B* is broad and regular, but deeper

Fig. 6.




Plan of crooked groove, *B B*. Length, over 120 feet; breadth, 6 to 12 feet; depth, 4 to 6 feet; descent, *f* to *g*, 10 feet.

than *A*. Three strata are visible: a thin flint bed; next above it the quicklime bed, which is the most valuable; and above this to the surface, a stratum that does not make lime, but is shipped to the iron furnaces for flux. Beneath, the rock is not worked. Over the whole was originally a thin layer of drift hard pan, *h h*. In this profile the crooked groove *B B*, has been worn down into the quicklime bed.

Profile Fig. 4, along the line *c d*, shows quite an irregular section, the north wall being vertical or projecting over the channel, the bottom of which is double and in the form of offsets or benches.

Fig. 5 shows how it is separated into branches, a view of which is seen in the photograph (Fig. 7) taken from near the point *e* (Fig. 6), looking southwest. Here the ends of all the channels,

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which converge at *e*, are closed with drift *h h*, and cannot be followed to the extremities.

From inspection it is evident that the sinuous groove *B B*, is due in part to the dissolving action of running water flowing out beneath an ice-cover. This flow is known to take place beneath

Fig. 7.

Upper End of Ice Groove *B*, Kelley's Island, looking Southwest.

all glaciers. It is muddy or gritty, according to the rock over which the movement takes place; sometimes carrying out gravel and small stones. The erosive effect of such streams is increased by pressure into the channel; the ice not being entirely rigid, but capable of being bent, to fit the depressions beneath it.

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## THE IRON ORES OF ALABAMA, WITH SPECIAL REFERENCE TO THEIR GEOLOGICAL RELATIONS. By EUGENE A. SMITH, of Tuscaloosa, Alabama.

### METAMORPHIC ORES.

THE question of the age of the metamorphic rocks of Alabama is still an open one, though in a few instances there is very little room for doubt that they are metamorphosed Silurian beds.

For the purposes of this paper it will be more convenient to treat of the ores of this region separately, without any reference to the age of the deposits in which they are found.

These ores are Magnetite, Titaniferous Magnetite, Hematite, Menaccanite, Limonite and Pyrite.

*Magnetite.* This is, next to Limonite, the most abundant ore in the region under consideration. Fragments of large size of granular magnetite, indicating a vein or bed of considerable thickness, have been found in Clay county. The bed itself has not yet been discovered. An analysis of this ore shows comparatively little phosphorus. (See Geolog. Report for 1874, p. 59.) In Coosa county and elsewhere, the magnetite is frequently met in masses showing crystalline faces of considerable size. In Randolph, Cleburne, etc., a slaty magnetite is not uncommon.

*Titaniferous Magnetite.* One specimen of this ore from Chambers county, where it is found rather abundantly, has been analyzed by Prof. Mallet. (Tuomey's 2d Report, p. 198.)

*Hematite*, in scales impregnating other rocks, may be found in very many localities in Coosa, Clay, Tallapoosa, and other counties; but no hematite in quantity has yet been observed.

*Menaccanite* or *titanic iron*, like the above, is known principally as an accessory ingredient in some of the rocks of this region. Particularly may be mentioned an arenaceous schist with flattened masses of this mineral occurring in Coosa and Clay counties. In Coosa county titanic iron seems to be rather common in some localities, in rounded, apparently water-worn fragments of considerable size.

*Limonite* is much more abundant than any of the ores above mentioned. In most cases these limonites appear to be "gossans," *i. e.*, the result of decomposition of beds of pyritous ores. In Coosa, Clay, Tallapoosa, Randolph and Cleburne counties, beds of

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this character of great extent *superficially*, but of unknown depth, have been examined. In many instances these ores appear to be rather mixtures of turgite with limonite, than pure limonites. An analysis of a sample of ore of this nature, gave me 9·79 water, ·76 phosphoric acid, ·61 sulphur, and 83·13 ferric oxide. Powder dark red.

Accumulations of limonite, resembling those of the non-metamorphosed Silurian beds, are quite common, especially associated with hornblendic rocks. The occurrence of compact, sometimes rounded or rolled fragments of limonite embedded in a deep red colored soil, in many places in the metamorphic region, reminds one forcibly of similar occurrences in the Quebec formation, to be mentioned farther on.

These beds have been comparatively little used, probably on account of their inaccessibility. In one place in Clay county, ore of this character was largely used some years ago in a Catalan forge.

*Pyrite.* Although *pyrite* can hardly be classed with the ores which are much used for the production of iron, yet on account of the occurrence of large beds in Clay county, of nearly pure pyrite, and in Cleburne county, of cupriferous pyrite, it deserves notice. Besides the localities mentioned, beds of pyrite as yet unexplored, have come under notice in many other places.

## Silurian Ores.

The ores of this formation are chiefly limonites, and lenticular and fossiliferous red ores. Other ores of more limited occurrence will be noted below.

1. *Potsdam Sandstone.* This rock contains frequently large amounts of iron, principally as *limonite* which appears as a superficial coating on exposed surfaces, or as small deposits filling the spaces between detached fragments of the sandstone.

Occasionally, the limonite is found in good specimens with botryoidal surfaces and radiately fibrous structure.

As a locality, the mountain west of Talladega may be mentioned.

In the Kahatchee hills in Talladega county, which are in good part made up of Potsdam sandstone, in places partially metamorphosed, we find the iron which impregnates the sandstone appearing as *specular ore* or as *magnetite.* Slaty masses of this half metamorphosed sandstone, impregnated with magnetite to such an

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extent as to exhibit very strong magnetic polarity, are common throughout these hills in the southern part of Talladega county. The *specular ore* is found in small quantities upon the sandstone, as flattened masses or scales.

2. *Calciferous. Knox Sandstone.* This, as a formation, is more or less ferruginous throughout, and small beds of *limonite* similar to those of the succeeding formation have been observed.

3. *Quebec. Knox Shales and Knox Dolomite.* The lower subdivision or *Shale*, like the sandstone just described, is ferruginous throughout, and local beds of limonite are not uncommon; but it is in the upper division or *Dolomite* that the great mass of the limonite of Alabama is to be found.

This Dolomite is the most massive calcareous formation in the state, and associated with it wherever it appears on a surface formation (viz., over the greater part of the area marked Silurian on the geological map), are accumulations of limonite in beds, pockets and isolated masses, embedded usually in red, sandy clay.

The ore is of concretionary nature, and geodes are often found which are lined inside with mammillary and botryoidal surfaces, with velvety goethite, etc. The solid masses are sometimes compact, breaking with more or less conchoidal fracture, liver brown in color; sometimes fibrous; sometimes made up of cylindrical columns of radiately fibrous structure; the outer surfaces being often coated with a dark, or nearly black glaze.

Whilst the masses of ore are usually embedded in a reddish, sandy clay, they sometimes appear covering the entire surface of hillocks, which seem to be made up altogether of ore. In the vicinity of the large accumulations of ore, the deep red colored soil is often partially covered with rounded solid fragments of limonite, like pebbles; and these limonite pebbles, where exposures are made by gullies, are seen embedded in the red clay to some depth, as wash ore.

Prof. Frederick Prime, jr., of the Pennsylvania Geological Survey, in describing the ores of the same geological horizon in Pennsylvania, distinguishes between those which occur in place and those which have been washed into depressions in the limestones, the former being almost invariably associated with damourite slates or clays.

In this state, whilst clays similar to those formed by the decomposition of the damourite slates of Prof. Prime are abundant in

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most of our ore banks, the study of these deposits has not been pushed so far as in Pennsylvania, so that it is at the present time impossible to say in what localities the ore is in place and in what localities it has been moved. Prof. Prime says "A marked peculiarity with respect to the occurrence of brown hematite ore in the damourite slates or clays, is the lack of uniformity with which it is distributed. What seems to be a solid bank of ore may at a depth of two or three feet be replaced by clay or slate containing no ore whatever. The miner can have no clew as to what may be six inches in front of his pick." If there be such uncertainty in the distribution of the ore in place, the uncertainty is still greater in the case of ores which have been washed into cavities in limestone.

In the ore deposits of the Quebec Dolomite, in Alabama, it is difficult, if not impossible, to discover anything like stratification in the masses of ore, and it is almost equally difficult to trace any connection between the distribution of the ore banks and the stratification of the formation with which they are associated, beyond the fact that they follow in general the outcrop of the Dolomite, sometimes over one series of beds in this formation and sometimes over another.

*Source of the iron.* There seems to be very little doubt that the limestones and chert of the Quebec Dolomite, and the Silicious group of the Sub-carboniferous formations, have by their disintegration furnished the iron; for most of the ore occurs mingled with the sands, clays, broken masses of chert and other débris which have evidently come from the disintegration of cherty limestones. Nearly every hillock, upon the two formations mentioned, shows more or less of iron ore as surface specimens, and many of the sandy clays have the deep red color due to the peroxide of iron. Where the iron has accumulated we have the ore banks.

The exact mode of formation of these ores and of their accumulation in banks is still imperfectly known, and I desire to call attention to certain points of resemblance between the ferruginous rocks which characterize our Southern or Stratified Drift, and the ores of the Quebec and Sub-carboniferous formations, and to their intimate association in certain localities, in the hope that some light may be thrown upon doubtful points in the history of our ore deposits.

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(1.) In many of the beds of clay of the Stratified Drift may be seen concretionary masses of limonite in process of formation. These clays are often mottled with irregular streaks of the iron oxide, distributed throughout the whitish or bluish clay, just such an appearance as one would expect if the clay had been impregnated with iron, which gradually became concentrated in these irregularly-shaped bodies leaving the rest of the clay free from the iron; or if the solution of iron, in penetrating the clay bed, had followed devious ways through it, and indurating, had thus formed these irregular limonite figures in the clay. So in the ore banks, it is not uncommon to find the sandy clays sometimes very little colored, and the iron almost entirely concentrated in the geodes, etc., of which mention has been made above.

(2.) Any one familiar with the region of this state, which is covered with the Stratified Drift, cannot have failed to notice the large amount of iron contained in some parts of it, and showing itself as a cement holding together grains of sand and often pebbles of considerable size, in this way forming the ferruginous sandstones and conglomerates or pudding-stones, which are so abundant. Nearly every considerable hill in the Drift region will be found to owe its present height to a protecting cap of such ferruginous sandstone. The iron solution, in sinking through the loose sands, meets with some impervious stratum, is arrested, and the hydrated ferric oxide binds together the grains of sand and pebbles. The ferruginous sandstones thus formed have various shapes; plates, tubes, hollow spheres filled with sand or ochre, and other forms similar to the calcareous concretions known as *Loess Kindchen*. Occasionally, on these sandstones are noticed places where the cementing material (limonite) is nearly free from sand or other admixture, and thus forming a fair ore of iron. I know of only one locality in Alabama where the ore of the Drift has been utilized, viz., near Vernon in Lamar county. It seems usually to be too much a mixture of sand and hydrated ferric oxide, with a predominance of the former, to be of use.

(3.) In the territory of the Quebec or Knox Dolomite, but only where this formation is partly covered with Drift, I have noticed occurrences which I mention here, on account of their bearing upon the question which we are considering.

(*a.*) In Bibb county, on the property of the Ashby Iron Company, are ore banks which lie adjacent to the Coal Measures. The

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Knox Dolomite, over which the ore occurs, is separated by a fault from the Coal Measures, but the line of demarcation between the two is extremely difficult to draw, since both formations are partially hidden by the pebbles and sand of the Drift.

Near the most westerly of these ore banks (*i. e.*, that nearest the Coal Measures), a capping of ferruginous sandstone, and sometimes of a ferruginous conglomerate, similar to the ferruginous rocks which are so characteristic of many of the hills of the Drift, is found covering the summits of hills of Carboniferous sandstone. Here, though the hills are mainly of sandstones of the Coal Measures, the superficial covering of pebbles and ferruginous rocks is probably of much later date.

However this may be, on one of the hills which has this capping of ferruginous sandstone or conglomerate, too silicious to be of value as an ore of iron, an improvement in the quality of the ore may easily be observed as one descends the hill, and about half way down the ore has been regularly worked. At this point the surface ore has frequently grains of sand, and in some cases, quartz pebbles enclosed in it; but by excavating a few feet, very excellent qualities of ore, fibrous, ochreous, and compact (liver ore), are brought to light.

Analyses of average samples of several varieties of ore from this and two other similar localities in the vicinity, show: —

| | 1 | 2 | 3 | 4 |
|---|---|---|---|---|
| Metallic Iron, | 56·10 | 57·91 | 51·96 | 55.05 |
| Phosphorus, | 0·45 | 0·24 | 0·58 | 0·57 |
| Insoluble silicious matter, | 6·04 | 3·06 | 7·84 | 5·61 |
| Water, | 10·49 | 7·41 | 12·44 | 12·72 |

All these ore banks, as has been stated above, lie at the foot of hills of Coal Measures sandstone, adjacent to the fault which separates this from the Quebec formation. The occurrence together of limonite, and of the pebbles and ferruginous rocks, if not of the Drift, at least undistinguishable from similar materials of that formation, has already received notice. The analyses *from average samples* show the limonite to be of very

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good quality, and with no unusually large amount of silicious matter.

(*b.*) In Bibb county, at a distance of several miles from the Coal Measures, and where the country rocks are of the Quebec or Knox formation bordered by the Trenton, are found, especially covering the hill-tops, rounded pebbles of the Drift, along with many fragments of the ferruginous sandstone, apparently also belonging to the same formation. In this locality occurs a bank of limonite of compact and fibrous varieties. The rounded pebbles, ferruginous sandstone, and ferruginous conglomerate of the Drift (formed of pebbles cemented by hydrated ferric oxide) cover the ground. A ferruginous sandstone is here one extreme of a series, of which pure compact and fibrous limonite, with little or no admixture of sand grains, is the other. If the pebbles, ferruginous sandstones and conglomerates on the one hand, and the limonites on the other, do not belong to the same geological period, it is difficult, if not impossible, to draw any line between them. That the pebbles and ferruginous sandstones are to be assigned to the Quatenary formation may be inferred from the fact that, from this point southward, the whole country is more or less completely covered with similar materials which are undoubtedly of that age.

An average sample of ore from this locality shows

| | |
|---|---|
| Metallic Iron, | 50·07 |
| Phosphorus, | 0.35 |
| Silicious matter, | 14.11 |
| Water, | 10·49 |

like the others above, a fair specimen of limonite.

(*c.*) The anticlinal valley, which separates the Cahaba from the Warrior Coal field, has the Quebec or Knox Dolomite for its principal surface formation, and associated with this here, as elsewhere, are great banks of limonite. The pebbles, ferruginous sandstones and other materials of the Drift, cover partially the lower fifteen or twenty miles of this valley, at first (northwards) sparingly and in isolated patches, but eventually hiding completely not only the Silurian strata of the valley, but also the Coal Measures on each side. In very many localities in this lower (southwestern) part of the valley, the association of ferruginous sandstone and pebbles with *limonite* may be observed. In most places where the great ore banks occur, the surface specimens are generally ferruginous sandstone and conglomerate, with compara-

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tively little hydrated ferric oxide. The sandstone, as is so often the case, is found in plates, tubes, hollow spheres, etc. In many instances the sandstone of the usual Drift type may be seen, together with tolerably pure limonite of fibrous texture, needle ore in fact, both upon the same specimen.

One familiar with the ore deposits, only where the Drift was absent, would not expect to find good ore where such sandy ferruginous rocks form the surface specimens, and yet some apparently of the most extensive and valuable ore banks of this valley are introduced, so to speak, by such surface rocks.

An ore from this locality shows

| | |
|---|---|
| Metallic Iron, | 50·68 |
| Phosphorus, | 0·12 |
| Silicious matter, | 9·80 |
| Water, | . . . |

like the others also, a fair sample of limonite.

In all the above described occurrences we may notice (1) the association together of sand, pebbles, ferruginous sandstones and conglomerates of Quaternary age, with limonites of the usual composition — *typical brown ores* indeed. (2) An association so intimate that every gradation between the limonites and the sandy ferruginous rocks may be observed. (3) That upon one and the same hand-specimen may often be found fibrous limonite free from sand, along with ferruginous sandstone containing comparatively little iron. (4) This association, so far as yet known, has been observed, with one exception only, in terranes underlaid by the Quebec or Knox Dolomite of the Silurian and by the Subcarboniferous formations, and only in the southern and western parts of these terranes where they adjoin the great belt of pebbles, sands, etc., of Stratified Drift (sometimes as much as one hundred feet in thickness), which hides everywhere in Alabama the junction of the Paleozoic and Cretaceous formations.

The conclusions which seem to be justified by the facts given above are that, whilst the Quebec or Knox Dolomite and the Subcarboniferous formations have furnished the *material* of most of our limonites, the *time* of the formation of *some* of the ore banks as we now find them, is the same as that in which were formed the ferruginous sandstones and conglomerates, usually assigned to the Stratified Drift, and as other limonite banks differ from those above described, only in the absence of the pebbles, sands, etc., of

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Quaternary age, over the territory in which the limonite occurs, the formation of these banks also may reasonably be referred to the same period; and whatever agencies have been active in the formation of the ferruginous sandstones and conglomerates usually ascribed to the Drift, the same have also been instrumental in forming part, at least, of the limonite with which they are so intimately associated.

Finally, I would not be understood as holding that the formation of the ferruginous sandstones and conglomerates, which characterize the Drift deposits, was confined to a single period. It seems much more probable that the cementing together of the Drift sands and pebbles by hydrated ferric oxide, whilst it may have *begun* soon after the deposition of the Drift, has been going on ever since. In fact, the formation of hard ledges in recent cuts, through ferruginous Drift sands, shows that the induration of these iron-bearing sands is going on now whenever the circumstances favor it.

So in the case of the limonites—whilst most of that which is found in our ore banks may have been formed long since, yet wherever a ferruginous solution from precipitation, evaporation, or other cause, deposits its iron, we have the conditions for forming an ore of iron, limonite, ochre, or other ore. It may be that even now, every rain which falls upon the ferruginous, sandy clays, in which most of our limonites are embedded, may help in the formation of limonite which at some future day may be utilized in the blast furnace.

4. The next horizon, at which we find an iron ore, is between the Quebec or Knox Dolomite and the Chazy limestone. The ore is a lenticular red hematite, not to be distinguished by the eye from a similar ore occurring in the Clinton formation.

This occurrence, *new to Alabama, and now first described*, is a limited one so far as yet known; it has been found only in the vicinity of Pratt's Ferry in Bibb county. The thickness of the bed, which lies conformably beneath Chazy limestone with characteristic fossils, is about four and a half feet. The ore contains traces of fossils, but none well enough preserved for identification have yet been found.

5. *Niagara.* In the *Clinton* group of this formation the fossiliferous red hematite or dyestone ore forms a persistent feature in Alabama as well as in Tennessee and the states farther north-east.

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The ore is usually found in a ridge on each side of the anticlinal valleys which separate the Warrior and Cahaba Coal-fields, and which, like Wills' and Murphree's valleys, separate parts of the Warrior field from each other. These ridges are composed of the rocks of the Clinton, the Black Shale (Devonian), and the Cherty portions of the Sub-carboniferous groups.

In Roup's and Jones' Valley the red ore ridge on the western side of the valley is *duplicated*, and both the ridges contain good ore.

The quality and amount of the ore vary from place to place along the outcrops. Analyses have been published in the Geological Report for 1876, which show the general character of ore in the vicinity of Birmingham.

At the Oxmoor Furnace, the only one in the state now using the red ore, it is mixed with limonite from Roup's Valley. The *Red Mountain* ridges, as they are called in Alabama, extend from near Scottsville in Bibb county, northeast into Georgia, in one series of valleys, whilst other shorter valleys have also their ore ridges.

6. In the vicinity of Columbiana, in Shelby county, there is an occurrence of red ore which differs from the ordinary ore of the Clinton group in being compact, not composed of flattened grains, showing no trace of fossils, and in having embedded grains of pellucid quartz. This ore occurs in a ridge similar in some respects to the Clinton ore ridges, but the absence of fossils has thus far prevented a determination of the age of this particular deposit. The ore outcrop has been traced along the entire length of this ridge, a distance of several miles.

### Carboniferous Ores.

1. *Sub-carboniferous.* The lower or Silicious group of this formation has in Alabama many points of resemblance to the Knox or Quebec Dolomite, mentioned above. Both formations are widely distributed, both are characterized by the occurrence in them of cherty limestones, and both are iron-bearing.

The chert of the Sub-carboniferous is bedded, that of the Knox or Quebec, concretionary.

The ore is limonite and resembles that of the Quebec formation in its occurrence, and in the general appearance of the ore masses, which are concretionary, geodes, etc. But in this formation the ore is more generally found associated with the broken-down

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fragments of the chert beds, and perhaps not so often in reddish sandy clays, as is the case with the Quebec Dolomite limonite. It also often includes masses of chert which are sometimes completely decayed to a white, chalky looking powder.

As to the extent of the ore beds and the character of the ore, it may be said that upon most of the cherty ridges of this formation in the anticlinal valleys of middle Alabama, the limonite is found; that it is often too silicious from included chert, and frequently holds too high a percentage of phosphorus to be profitably worked. In Russell's Valley in Franklin county, North Alabama, there are banks of limonite of great extent and with high percentage of metallic iron. The occurrence is in every respect similar to that of the limonite banks of the Knox Dolomite, the ore being embedded in red clay soil, showing no trace of stratification. The ore here shows the usual varieties, fibrous, compact, ochreous, etc.

A Catalan forge was once supplied from one of these ore banks, but at the present time, there is, in Alabama, no furnace using the ore from this formation. Throughout the valley of the Tennessee in North Alabama, there are numerous occurrences of ore banks of considerable superficial extent, the ore of which may upon closer examination be found fit for use. This is all the more probable from the fact that in the adjoining state of Tennessee the ore banks of the Sub-carboniferous have been extensively worked.

Concerning the *age* of the ore as now encountered, the remarks upon the age of the Knox Dolomite ores will, in great measure, apply here. The association of the ore with rounded pebbles, etc., is common here also. Prof. Safford, who has examined the ore banks occurring upon this formation in Tennessee, is of the opinion that the *source* of the iron may be looked for in the ferruginous chert, whilst the formation of the ore masses, as we now find them, has probably taken place at a comparatively recent date, perhaps subsequent to the time when the pebbles and sands, associated with much of the ore, were deposited.

2. *Coal Measures.* The two ores found in this formation are *clay iron stone* and *black-band.*

The Coal Measures of Alabama have been so little explored that occurrences of these ores are not much known. Analyses have been published, however, of very good clay iron stones from Jefferson and Walker counties. The ore is not yet worked.

At New Castle Station on the South and North Alabama R. R.,

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there is exposed a bed of black-band ore one foot in thickness. This bed has been leased to the Eureka Iron Company at Oxmoor.

The Warrior Coal seam, at Warrior Station on the same railroad, has a capping of cannel coal and black-band, two feet, four inches thick.

## Ores of Later Formations.

There is very little to say of ores in any of the formations later than the Carboniferous. I believe that nothing like ore banks occur in any except the Drift.

This formation is remarkable for the large amount of iron which it holds. Most of the sands and clays, especially near the Paleozoic terranes, are colored deep red, orange and yellow, with the hydrated peroxide, which is often found in local accumulations somewhat after the fashion of the ore banks of the older formations, but generally mixed with much sand and other Drift materials. Anything like a fair limonite I have never seen in this formation, at any great distance from the Silurian or other older formation from which the iron seems in all probability to have been derived, except in the case of the ore bank near Vernon, Lamar county, already mentioned above. The usual occurrence of the ferric oxide in the Drift is, as has been stated above, rather as a cement holding together grains of sand and pebbles, and widely distributed coloring the sands, etc., than as a true ore.

It seems to be a fact that the farther we go southward the less iron do we find in the Drift, and in the lower part of the state, in the pine woods, the prevailing color of the sands is either white or ashy, showing comparatively little iron. On the other hand, that bed of Drift materials which hides the line of junction of the Paleozoic and Cretaceous formations is commonly deeply colored with iron.

In conclusion, a few words may be said concerning the ore deposits upon which the iron industry of the state will chiefly depend in the future, and particularly in regard to their *extent*.

Very little, next to nothing, is known of the iron ores of the Metamorphic region. The inaccessibility of these beds will stand in the way of their development, probably for years to come.

The Sub-carboniferous limonites also have received very little attention, and it is from the ore banks of the Quebec and the red hematite beds of the Clinton formations, that most of the material



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to supply our blast furnaces, for many years, will probably be obtained.

The Clinton ores, occurring as they do in stratified deposits, may easily be examined and their quality and quantity ascertained.

The case is, however, quite different when we turn to the Lower Silurian and Sub-carboniferous limonite banks. From the very mode of formation of these banks, it will be seen that nothing but an *actual section* can give with any certainty their thickness, since these deposits are in no way connected with the stratified beds, but are found as local pockets or strings of pockets.

It is common to hear the term "inexhaustible" applied to these ore banks, and if their thickness corresponded in any way with their superficial extent, this term might not be so far wrong, if we mean by "inexhaustible" to say, that the bank holds ore enough to supply a blast furnace for many years.

The fact is, we know scarcely anything of the capacity of any of our great ore banks. That of the Shelby Iron Works has been perhaps the most thoroughly explored, and it must be admitted that in this case, as also with the banks of the Woodstock Furnace, the supply of ore shows no sign of exhaustion. From these two instances, in which the furnaces have been in blast for many years, we might be justified in inferring that, in other cases where the accumulations of ore are of great superficial extent, the amount of ore below the surface is correspondingly great.

An inference, however, is not a certainty, and certainty can be had only from actual thorough exploration, and this is what almost none of our ore banks have yet received.

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ON SOME CHARACTERISTICS OF THE VEGETATION OF IOWA. By JOSEPH CHARLES ARTHUR, of Charles City, Iowa.

[ABSTRACT.]

I.—Iowa lies on the border of the western plains, but has a flora largely identical with that of the Eastern States. Over seven-eighths of the indigenous species extend east to Indiana, while

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only about twenty are exclusively trans-Mississippian. Some peculiarities of the flora are very prominent. The more robust habit, and firmer growth of tissues is a matter of even casual observation. This is well illustrated by the tumble-weed (*Amarantus albus*, L.), a somewhat succulent and not very troublesome weed in the East, which, under the influence of western surroundings, becomes stout and rigid, and with the advent of frosts drops its leaves, breaks away from the earth, and is ready to be rolled and tossed by the wind. The entire absence of *Ericineæ*, or true Heaths, and of *Sphagnum*, and of all Lycopods, deserves attention; and other differences might be enumerated.

It is proposed to call attention to a few of these, and to suggest some explanations.

TABLE SHOWING THE PERCENTAGE OF SPECIES AS COMPARED WITH THE WHOLE NUMBER OF INDIGENOUS PHANEROGAMOUS PLANTS OF EACH REGION.

| | U. S. | D. C. | MICH. | OHIO. | ILL. | IOWA. | NEB. |
|---|---|---|---|---|---|---|---|
| Ericaceæ | 3·0 | 2·5 | 2·3 | 2·3 | 1·2 | 0·3 | 0·7 |
| Orchidaceæ | 2·5 | 2·4 | 3·4 | 2·9 | 2·0 | 1·2 | 1·9 |
| Cyperaceæ | 10·9 | 9·2 | 10·6 | 10·5 | 9·5 | 6·9 | 9·5 |
| Gramineæ | 7·8 | 8·8 | 7·8 | 7·6 | 7·5 | 7·5 | 8·6 |
| Glumaceæ | 18·7 | 18·0 | 18·4 | 18 1 | 17·0 | 1.44 | 18·1 |
| Trees | 6·2 | 7·5 | 7·4 | 7·4 | 6·8 | 6·7 | 4·2 |
| Ligneous Flora | 16·0 | 16·7 | 17·1 | 16·5 | 14·9 | 12·9 | 9·8 |
| Ranunculaceæ | 2·4 | 2·4 | 3·0 | 2·9 | 2·8 | 3·8 | 2·7 |
| Compositæ | 12·7 | 13·1 | 12·0 | 12·6 | 13·7 | 15·4 | 14·7 |
| Annuals | 13·0 | 12·0 | 9·5 | 11·6 | 14·9 | 16·5 | 14·0 |

The above table gives the percentage of a few prominent orders and classes of plants as compared with the whole number of indigenous flowering species of the respective regions named. Gray's Manual, fifth edition, has been taken as the basis of the computation, and especially to determine the indigenous, the annual, the ligneous, and the arboreous, species. The data for the states named have been secured from a catalogue of plants of the District of Columbia and vicinity, by the Potomac-side Naturalist's Club, published in 1876; a catalogue of Michigan, by Elmore Palmer, 1877; of Ohio, by H. C. Beardslee, 1874; of Illinois, by H. N.

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Patterson, 1876; of Iowa, by J. C. Arthur, 1876, with addenda of 1877–8; and of Nebraska, by Samuel Aughey, 1875. It would undoubtedly have been more satisfactory to have taken a catalogue of one of the New England or Middle States, instead of the District of Columbia, to represent the character of the eastern flora. But no such catalogue not otherwise objectionable was at hand; and, as the *Flora Columbiana* is made to include the "vicinity," it is thought to give a fair exhibit. Only such species were counted among the trees as reach eighteen feet or more in height.[1]

A discrepancy in the table that it is necessary to notice is the comparatively small percentage of *Cyperaceæ* and *Gramineæ* given for Iowa. The glumaceous plants have a wide range, and are generally well represented, both by species and individuals. Considerably over nine-tenths of the surface of Iowa is exposed grassy prairie, giving such plants remarkable advantages. Only one explanation of the deficiency shown in the table seems probable; this class of plants has not yet received due attention from Iowa botanists. The conclusion is confirmed by the relation, as given in the table, between the two orders (*Cyperaceæ* and *Gramineæ*); that for Iowa is inverted, as compared with the other regions.

Although the table is not as comprehensive as could be desired, it is sufficiently so to exhibit a few very interesting facts. It shows that the *Ericaceæ* gradually diminish as we pass westward, excepting a slight increase in Nebraska. The *Orchidaceæ* show an almost parallel distribution. The latter, however, reach their maximum in Michigan, a state with an abundance of Tamarack swamps and other moist and protected situations, such as are congenial to most members of the order. The comparative number of glumaceous plants seems to be quite constant, not varying far from eighteen per cent. of the total number of phanerogamous plants, except in Illinois and Iowa, which latter discrepancy has been explained. The ligneous flora constantly decreases toward the west, with the exception of Michigan, where, as is to be expected, the maximum is reached. The arboreous portion shows an uninterrupted decrease westward. The *Ranunculaceæ*, *Compositæ* and annual plants, give the largest number for Iowa.

[1] The standard taken by Dr. Vasey in preparing the United States' Exhibit at the Centennial.

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TABLE OF THE NUMBER OF INDIGENOUS SPECIES OF CONIFERÆ IN EACH REGION.

| | U. S. | D. C. | MICH. | OHIO. | ILL. | IOWA. | NEB. |
|---|---|---|---|---|---|---|---|
| Coniferæ..... | 21 | 5 | 15 | 11 | 11 | 4 | 6 |

Although the *Coniferæ* are represented by too few species to be included in the previous table, they have an interesting distribution. The four species found in Iowa are *Pinus Strobus*, *Juniperus communis*, *J. Virginiana*, and *Taxus baccata*, var. *Canadensis*. Of these, *Juniperus Virginiana* is common to all the areas mentioned; *Juniperus communis* and *Taxus baccata* are absent from the *Flora Columbiana*, the region being too far south for them; and *Pinus Strobus* and *Taxus* are not in Nebraska.

With the exception of the *Glumaceæ* and the trees, it is largely true of the plants mentioned in these tables, although no necessary sequence, that a direct and considerably increasing ratio exists between the number of species and the number of individuals.

To recapitulate: *Ericaceæ*, *Orchidaceæ*, and *Coniferæ* reach their minimum in Iowa, and the *Glumaceæ* doubtfully so; ligneous plants, including trees, farther to the west; while *Ranunculaceæ*, *Compositæ* and the annuals, reach their maximum in Iowa.

II.—*Physiography of Iowa as related to the Vegetation.* This portion, which consists of condensed meteorological and other statistics, must be almost entirely omitted, as it is not possible to bring the matter into a sufficiently brief space for the present abstract. The following items are very important.

The relative humidity sometimes drops to twenty-six per cent., and is not infrequently at or below forty per cent. A heavy shower does not necessarily imply saturation of the air, at such times the relative humidity being from ninety-three per cent. upwards. This is not to be interpreted as indicating a meagre rainfall, for Iowa generally receives a greater depth of rain than eastern states in close proximity to the Atlantic, and, with the frequent exception of a portion of the winter, it is quite equably distributed through the seasons.

From sixty to seventy-five changes of temperature of thirty degrees or more, within twelve hours, may be expected during each year. On February 14, 1876, at the Agricultural College, a rise of

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forty-eight degrees was recorded, and at Ft. Dodge, in August, 1878, the thermometer dropped thirty-four degrees in less than eight hours.

III.—*Conclusions.* The richness of the soil insures a profuse and luxuriant growth, but adverse climatic influences prevent the attainment of great size. This is particularly obvious among forest trees; very large individuals being rare. The general coarseness and roughness, and the firmer tissues of western plants, can doubtless be ascribed to the severities of climate.

The level character of the surface of the state excludes all plants requiring high latitude or altitude, with many others, especially those that thrive in very cool, shady ravines.

Not a *Sphagnum* nor a *Lycopodium* occurs in the state, which, in itself, indicates the non-occurrence of orchids and other plants growing in similar situations.

The annual rainfall of Iowa is entirely sufficient, unless in the northwestern part, to support dense forests with their accompanying vegetation, as Dr. Gray has pointed out in a recent article.[2] The dry, sweeping winds from the northwest, which are usually accompanied by clear skies, with the remoteness of large moisture-dispensing bodies of water, and the occasional scorching south winds, all conspire to give to Iowa the dry climate characteristic of the interiors of great continents. The effect of this dryness is augmented by the rapidity with which the soil loses its moisture, it having but little protection from forests or hills. The low rainfall of winter, and the not infrequent drouths of summer also add to the general effect. Couple all this with the sudden and great changes of temperature, and it goes far toward explaining the deficiency of ligneous plants, and of *Ericaceæ*, *Orchidaceæ* and *Coniferæ*. Then there is to be added the yearly range of temperature: many plants which receive no injury from the excessive heat of summer are killed by the cold of winter, and *vice versa*.

We have suggested reasons for the deficiency which, the tables show, exists in certain classes of Iowa plants, and now pass to the classes noticeable for their abundance.

The habitat of *Compositæ* is largely in open ground. Iowa, nine-tenths prairie, offers these plants unusual advantages, to which are added the natural hardiness of the order, and the often

[2] Am. Jour. Sci. and Arts, Aug., 1878.

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roughened or hairy exterior, enabling them to withstand extremes of temperature. Annuals, not being affected by the rigors of winter, have in consequence more chances of success than perennials. Many do not require the entire summer to complete their growth. Of such, some, as the *Drabas*, prefer a moist, cool climate, and find suitable conditions during spring; while others, better adapted to a hot and dry climate, *e. g.*, the *Euphorbias*, are brought into life by the high temperature of early summer, and grow with great luxuriance during the tropical heat of July and August.

Some plants seem able to alter their habits considerably in order to suit themselves to changed conditions. Such are some of the members of the order *Ranunculaceæ*. The *Ranunculus Cymbalaria* is a notable example. Although a maritime plant, or confined to "salt springs" in the interior, according to Gray's Manual, it is not infrequently found on the exposed prairies of Iowa. The writer has seen it covering a plain of a large number of acres near Ft. Dodge. Other instances could be given.

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## Changes Preliminary to Cleavage in the Egg of Clepsine.

By Charles O. Whitman, of Newton Highlands, Mass.

[ABSTRACT.]

The embryology of one of the higher animals may be said to embrace the history of two successive periods, viz., the *preembryonic* and the *embryonic*.

The pre-embryonic period includes the growth and maturation of the egg, impregnation, nuclear changes preparatory to cleavage, the cleavage, the formation of a hollow sphere (*Blastula*, Häckel) from the cleavage products, and the transformation of this sphere into a double-walled sac (*Gastrula*, Häckel; *Planula*, Lankester).

The second, or embryonic period, is introduced by the formation of a *primitive stripe*, formed by the concrescence and concomitant thickening of the two halves of the Gastrula-orifice. During this period the parts of the future animal are sketched out and more or less completely developed.

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Older embryologists devoted their attention almost exclusively to the embryonic phases of development; it is only within the last seven or eight years that pre-embryonic phenomena have begun to receive the attention they deserve. It is some of these phenomena to which I shall now ask your attention, making the egg of Clepsine the point of departure.

Clepsine marginata, as many of you know, belongs to the leech family. It is a fish parasite, measuring about one inch when fully grown. It is found in ponds or small brooks where the current is gentle or imperceptible, attached to leaves, stones, fallen branches, etc. The eggs of this species vary in number from twenty to two hundred, according to the size of the specimen and the amount of food it has taken, and are generally attached to the leaves of living plants.

This leech, like others of the genus, has the interesting habit of remaining over its eggs till they hatch. At the time of hatching the young are little developed, and entirely helpless. Their protection is very curiously provided for. Soon after hatching they become attached to the ventral side of the parent, and are thus borne about until fully developed. During this time they receive no food from the parent, as shown by the fact that they develope quite as well when separated from the mother; their only food is the nutritive yolk furnished by the egg.

When, at the end of from two to three weeks from the time of hatching, this store of food is exhausted and the young are developed, the parent seeks to attach itself to a fish. This done, the young leave the mother and creep over the fish and take their first meal.

*Origin and Growth of the Egg.*—The egg of Clepsine, like that of any other animal, is derived from a simple cell. This primary egg-cell is a spherical body inclosed within a thin transparent membrane, and is composed of three concentric parts, viz.—the protoplasmic body, the nucleus, and the nucleolus. The first step in the development of this cell into the proper ovum, is signalized by the accumulation of fine, opaque granules in the body of the cell, around the nucleus. These granular precipitations are white when seen by reflected light, and exhibit a lively tremulous motion when brought into contact with water—the so-called Brownian movement. They multiply rapidly with the growth of the egg, and soon render the entire ovum opaque. When the egg is about

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half-grown some of these granules begin to expand by a simple process of growth, thus giving rise to the yolk-spheres. The color of the egg, which may be either white, yellow or green, depends upon the color of the yolk-spheres. As these granules and spheres have a secondary origin, as compared with the semi-fluid protoplasm in which they are imbedded, they have been termed by Van Beneden, *deutoplasm*. The origin of the deutoplasmic elements in the bird's egg or in the reptile's egg is, according to the investigations of Gegenbaur, precisely the same as in the egg of Clepsine, the only difference being, that in the former the yolk undergoes a farther differentiation into white and yellow yolk.

The full-grown egg of Clepsine measures about ·80$^{mm}$ — about the size of a small pin-head — the nucleus about ·08 to ·09$^{mm}$, and the nucleolus only about ·01$^{mm}$.

*Transformation of the germinal vesicle.*— Shortly after the maturity of the egg, the nucleus, or germinal vesicle as it is usually called, undergoes a remarkable transformation. The cause of this change is as yet unknown. It is simply known that the membrane of the germinal vesicle dissolves, the nucleolus becomes invisible, and that the whole or a part of the vesicle assumes the form of a spindle, around the two poles of which the protoplasm of the yolk becomes arranged in radial lines. This form of the nucleus, the precise origin of which was first made known by Auerbach, in 1874, was called by him "*karyolytic figure*," but has since been more appropriately termed *amphiaster*, or double star, by a distinguished naturalist of Geneva, Dr. Hermann Fol.

The spindle of the amphiaster, or bistellate figure, is composed of fibres curving outward, and converging to a point in each pole of the amphiaster. These spindle-fibres appear to differ in no essential respect from the radial polar lines; but in the eggs of some other animals and in the eggs of plants, according to Bütschli and Strasburger, they form the most conspicuous part of the figure.

The entire figure bears a most striking resemblance to the picture produced in iron-dust under the influence of a magnet. This resemblance was at once remarked by Leuckart and others who have seen my preparations; but it is not quite perfect in one particular — that no curves appear in the radial polar lines. But it is to be remembered that these phenomena have thus far been studied in microscopic sections of eggs that have been treated with various

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acids. It is quite possible that such curves do exist in the living egg, and that they become obscured or obliterated by treatment with acids and staining fluids. That we have here an electrical phenomenon is suggested not only by the general appearance of the amphiaster, but also by the deportment of the two poles during the process of cleavage. This amphiastral form of the nucleus is, as we shall see farther on, one stage in the process of self-division. It is a transitional phase by which one nucleus becomes two nuclei. This division of the nucleus generally results in the division of the egg, or the cell. As often as the cell divides, the amphiastral figure recurs. It is plain, therefore, that we have here a life-phenomenon of very great importance. It is a phenomenon of the deepest interest to the biological student, inasmuch as it stands at the very threshold of all organic life. It is common not only to all eggs, whether the eggs of plants or of animals, but also to all cells, whether embryonic cells or the cells of fully developed tissues. Its universality testifies to the unity of life.

*Quiescent state.*— After the formation of the amphiaster, the egg appears to maintain a quiescent condition, until, at the time of deposit, it is brought in contact with water. The proof of this fact was obtained in the following manner. I disturbed a leech that was laying its eggs, and, in consequence of the interruption, three eggs were retained in the ovary. The leech manifested no disposition to part with the remaining three eggs. At the end of forty-eight hours I cut the leech through the middle and thus liberated three eggs. To my surprise, I found them in precisely the same condition in which I had found the others two days before. The eggs were kept in both cases and developed in the normal manner. I have observed cases in which the eggs have been retained in the ovary four to five days after they were ripe for deposit. I am the more certain on this point, as I have been able to recognize this time with sufficient accuracy to exclude failure in every case where I made the experiment of cutting the leech to obtain eggs.

This quiescent condition, which can be maintained for at least two days in the egg of Clepsine, and which in the egg of the hen, according to Colasanti, may continue three weeks, and, in rare cases, even four weeks, without fatal injury, reminds one of the analogous fact that seeds can be preserved for years in a similar condition.

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*Polar Figure.*— Soon after deposit, the amphiaster moves towards the periphery of the egg in such a manner that one of its poles comes to lie exactly in the surface. This pole appears on the living egg as a small white spot; examined under a low magnifying power it shows a distinct radial structure. This polar figure is the outward expression of the radial lines around the external pole of the amphiaster. About twenty minutes after deposit there appears exactly in the centre of this figure a *pellucid spot*. This spot is free from deutoplasmic elements, but appears dark in consequence of the opaque background. This spot is the centre of the outer pole of the amphiaster. The polar figure not only reveals the position of the amphiaster, but also determines the main axis of the egg and at the same time the axis of the future embryo. It marks very nearly, at least, the place where the future mouth forms, and this pole of the egg may be therefore called the *oral pole*, and the opposite the *aboral pole*.

*Polar Globules.*— About thirty minutes after deposit an interesting change takes place. A well defined constriction appears in the equatorial plane of the egg, and passes by a sort of peristaltic movement toward that pole of the egg which bears the polar figure. In the course of ten minutes it passes off at this pole, leaving only a nipple-like protuberance. In a few minutes this protuberance is seen to be a transparent spherical body in process of liberation. Soon it is fully eliminated and lies between the yolk and the egg-membrane. Thirty minutes later a second body, like the first, is expelled, though accompanied by a much less clearly defined constriction. These two bodies are the so-called "*polar globules*," or "*directive corpuscles*." They are of very general occurrence in the eggs of animals as well as the eggs of plants, but the constriction attending their exit above described, has not been observed in any egg save that of Clepsine.

Whence come these polar globules? If we make sections of the egg, at the time the constriction is passing, parallel to the axis of the egg, we shall, if successful, obtain one section which includes this axis. In this section we shall find the amphiaster in process of division, and we shall see that the polar globule represents the external half of the amphiaster, together with a little of the protoplasm of the egg. It contains then nuclear substance and cell-protoplasm, in other words, is a genuine cell. The remaining half of the amphiaster is soon transformed

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into a complete amphiaster, which divides, and produces the second polar globule. The half of the last amphiaster which still remains in the egg is not changed into a third amphiaster, but assumes the form of one of the amphiastral poles. This stellate figure is the *female pronucleus*. Near the opposite pole of the egg a second stellate figure is seen. This is the *male pronucleus*. It owes its origin to a spermatozoon which has previously entered the yolk.

*Cleavage-nucleus.*—In the female pronucleus two small bodies are seen in close apposition, and in the male pronucleus, one such body. These bodies are nucleoli. The two pronuclei begin to approach each other, as if by reciprocal attraction, meet near the centre of the egg and here blend into a single nucleus—the cleavage-nucleus, so called because it is the nucleus which introduces the cleavage. The three nucleoli also meet in the centre of the cleavage-nucleus, but do not blend until about the time the cleavage begins. From this it appears that *impregnation consists in the complete fusion of corresponding parts of two sexually differentiated cells — the egg-cell and the sperm-cell.*

*Polarity.*—While the phenomena thus far described—polar figure, pellucid spot, and polar globules—have been confined to one pole of the egg, those which are to follow are repeated on both poles of the egg. A short period of uni-polar activity is followed by a longer period of bi-polar activity. In the latter period the pole thus far active, still asserts its preëminence by taking the lead in actions that repeat themselves later and more sluggishly on the opposite pole.

*Polar rings.*—I have now to describe a phenomenon thus far known only in the egg of Clepsine. About one hour and thirty minutes after deposit, a shallow groove forms around the oral pole, and in this groove a transparent fluid collects, thus forming a conspicuous polar ring. Soon the equatorial side of this ring becomes denticulate or rayed. Ten minutes later a similar ring forms around the aboral pole, with rays from its equatorial side. The life of the egg at this time seems to be concentrated in this outward display of polarity. These rings begin to advance slowly, each toward the pole it incloses; meanwhile the rays have reached their maximum intensity and begin to dwindle. The oral ring advances in such a manner that it forms a constriction about the inclosed yolk, forming of this a kind of flat cap, or calotte.

The aboral ring does not strike deep enough to form a calotte,

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but seems to pass over the pole, forcing the inclosed yolk in towards the centre of the egg. This concentration of the rings continues until the aboral ring has the form of a disc, and the oral ring is pierced only by a slender column of yolk, on which rests the calotte. If we make a median section of the egg at this time, the aboral disc will present an oblong elliptical form, while the oral ring will appear as two separate parts, divided by the slender column of yolk which supports the calotte. Each of these parts has a sharply outlined cordate form. At this time the nucleus will be found a little eccentric, lying nearer the oral than the aboral pole. The three nucleoli have maintained their identity and relative positions thus far. A few minutes later the ring-substance loses its sharp outlines and begins to flow slowly towards the centre of the egg. At this moment the three nucleoli become invisible, and the nucleus assumes the amphiastral form already described. This amphiaster is much larger than those before mentioned, and its division will result, not in the production of a diminutive cell, or polar globule, but in the sundering of the egg into two nearly equal parts. It is interesting to note the behavior of the two poles of the amphiaster while the egg is dividing. The cleavage-groove begins first on the oral side of the egg—on that side nearest to which the nucleus lies. At the moment it begins we find that the two amphiastral poles are much farther apart than at first, and that the radial polar lines are much more intense. As the cleavage advances the two poles move still farther apart and the radial lines reach their maximum intensity. It is plain to see that the power which cleaves the egg resides in the nucleus. During the cleavage the two poles of the amphiaster act as if repelling each other, moving farther and farther apart and pulling the passive yolk elements with them. Thus they appear as centres of attraction for the yolk.

*Approach of the two cleavage spheres.*—As soon as the cleavage is completed, the two parts, instead of parting company, begin to approach, flattening against each other, until at length they together form a perfect sphere—as perfect as the undivided egg. The line of division is not, however, obliterated. Why do the two parts of the egg approach in this manner? This fact has been often noted and has been generally explained as the result of pressure exerted by the elastic membrane of the egg. This explanation, however, cannot apply to those cases where the egg lies at

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a great distance from its membrane, as in the case of many Molluscan eggs. I have good evidence that it does not apply in any case. I once liberated an egg from its membrane, and thus had an opportunity to follow the cleavage unhindered by the membrane. At the close of the cleavage the two parts touched each other only at a common point. Subsequently they began to approach, flattening on their contiguous faces, and finally formed together a perfect sphere. If we follow the two nuclei that have resulted from the division of the amphiaster, during the approach, we find that they take the lead in the movement. The cleavage as well as the subsequent approach of the cleavage-spheres are therefore due to nuclear influence.

"*Faltenkranz.*"—In conclusion I will call attention to some interesting radial phenomena which have never been satisfactorily explained.

Kleinenberg, in his well known work on the development of Hydra, states that the cleavage begins on one side of the egg, and that pseudopodial processes arise from the walls of the cleavage-groove, on this side. Metschnikoff observed folds on the walls of the cleavage-groove in the eggs of certain Siphonophores. As long ago as 1824 similar folds ("Faltenkranz," Reichert) were observed by Prévost and Dumas on the Amphibian egg. These folds were seen by von Baer, and more recently by Götte. They have been made a subject of special study by Max Schultze and Reichert. According to every explanation thus far offered of these folds, they should appear at right angles to the plane of cleavage in its entire circuit. This, however, is not the case. They appear only on one side of the egg, and instead of being at right angles to the plane of cleavage, they are at right angles only at the centre, diverging more and more towards either end of the groove, thus forming a kind of long star. From what has before been said, it is evident that the radial influence of the two poles of the amphiaster is greatest at the moment the cleavage appears, and that the cleavage begins on one side, rather than on all sides simultaneously, because the nucleus lies eccentrically—always nearer the side on which the cleavage begins. It seems, therefore, highly probable that these "processes" and "folds" are merely the outward expression of the radial influence of the nucleus.[1]

[1] For a more detailed account of the above phenomena, see Quart. Journ, Mic. Sci., July, 1878.

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## The Philosophy of the Movements of the Rocky Mountain Locust. By Charles V. Riley, of Washington, D. C.

Prior to the year 1876 very little that was accurate and reliable was known of the destructive locust that at times proves so disastrous to the agriculture of the trans-Mississippi country; while still less was known of its movements. Had any one been asked, even eighteen months ago, whence come the ravenous hordes that sweep over the more fertile country between the Rocky Mountains and the Mississippi, or whither they go, he could have given but a hypothetical answer. Late investigation has, however, materially altered the situation, and we are to-day in possession of much absolute knowledge both as to the habits, natural history and movements of this species. It is of the principles which underlie these movements that I desire briefly to speak; premising that any rule or law we may formulate, in biological science, will admit of exceptions, and can never possess the same fixed and absolute character that belongs to rules in the exact sciences.

In order to treat intelligently of this locust problem, the U. S. Entomological Commission has found it necessary to divide the territory affected by the destructive and migratory species (*Caloptenus spretus* Thos.) into three regions, as follows:—

### 1. The Permanent Region,

or native breeding grounds of the species. This region east of the mountains embraces (*vide* map which is reduced from one in the Report of the Commission) all that vast plains region of the northwest between latitude 37° and 52° and reaching from the mountains to about the 103rd meridian. This forms altogether the most extensive breeding ground and comprises an area *north* of the boundary line, nearly equal in extent to that *south* of the same. West of the main range the Permanent Region is represented by more restricted areas, comprising the Snake River, Cache, and the Malade Valleys. In all this vast region the climatic conditions and floral characteristics are similar. It is an extensive plateau from 2000 to 6000 feet above the sea level. The summers are usually dry and intense; the winters long and severe. The atmosphere is attenuated and the vegetation generally sparse —the ordinary plains, covered with short but nutritious grasses,

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now giving way, in moister sections and along water courses, to a ranker prairie growth; now to almost complete desert, where little else but the sage-bush and a few cacti will grow. In the main it is what naturalists term a sub-boreal region and our Rocky Mountain Locust is, in its natural range, a sub-boreal insect. It is a form, to use Dr. Packard's words, "purely characteristic of the central zoögeographical province of North America.

The extent of this territory is not less than 300,000 square miles, but it must not be inferred that the insect breeds uniformly over the whole. It breeds principally on the more moist and fertile plateaus, in the river valleys, in the subalpine mountain parks, protected slopes, cañons, etc.,—wherever, in short, vegetation is favored throughout the region. The valleys of the South Saskatchewan, Yellowstone and North Platte are the most extensive areas.

The prevailing winds in this region, east of the mountains, are conspicuously from the northwest during the season when the insect is attaining maturity and full wing-power, and it is from this region that emanate the destructive swarms that oversweep the lower Mississippi country. West of the main range and in portions of Montana and Wyoming, especially west of the Belt range, there is no such uniformity in the course of flight, and local influences more materially affect it.

The species is always to be found more or less numerously in this Permanent Region.

### 2. The Sub-permanent Region.

This is a comparatively narrow area bordering the Permanent Region on the east; into which area flights are made with considerable frequency. The insect is, consequently, quite often found here. It can also perpetuate itself in this region for several years, but in the end disappears.

### 3. The Temporary Region.

This, as the map indicates, embraces a large extent of fertile country lying all around the other two regions. It is the country which the insect visits and devastates at irregular intervals, in which it cannot permanently thrive and which it vacates within a year. This last statement holds absolutely true for all that portion east of the mountains and south of the 43rd parallel, but

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less true in the western border and in the extreme northern portion, which partake more of the sub-permanent character.

In this Temporary Region the species becomes diseased and cannot hold its own; and in the Mississippi Valley there is always a return migration of such insects as hatch within it and which do not perish from disease or enemies, ere getting wings — the direction being conspicuously toward the Permanent Region.

This return migration, which I first set forth in 1874, has been fully confirmed by subsequent experience, and especially by the elaborate records (numbering about 2000) published by the Commission in 1877. It commences in April in Texas and continues from more northern points till about the middle of July, and, as in the case of the invading movements from the Permanent Region, which occur from the middle of July on through the autumn, it is aided by the prevailing winds, which, during April, May and June, are pretty constantly toward the northwest.

There is a marked difference between the general migratory movement and the shorter, less constant flights made in different directions in quest of food.

## Causes of Migration.

There have been various theories advanced as to the cause of the migratory movements of the locust, but we should not look to any one, but to several causes that may either singly or combinedly produce the result. Were I asked for any single explanation or predisposing cause, I should answer, *Excessive Multiplication;* for the others are mostly secondary, or but consequences of this one. The following are worthy of consideration: —

1. *Hunger.* — Whenever food is lacking, whether by virtue of excessive drought that is not uncommon in that portion of the country, or through such excessive multiplication of the species that all vegetation is devoured before the insects have completed their life-course, there must needs be the strongest incentive to migrate, as is well known to be the case, under like circumstances, with many animals normally non-migratory.

2. *The Procreative Instinct.* — My observations lead me to the conviction that there is a natural tendency in the species to seek for fresh breeding grounds away from the location of birth; and even were there no tendency of the kind, we may find a sufficient incentive for movement from one place to another during the season



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of procreation, in the well known salacious habits and ardor of the males. Whenever the insect is excessively abundant the females are greatly disturbed and annoyed during the act of oviposition, as several males will be constantly attending her.

3. *Increase of and Annoyance from natural Enemies.* No one, who has witnessed the excessive abundance in which some of these natural enemies of the locust at times prevail, can for a moment doubt that they often prove a valid cause of migration. The Tachina-flies, especially, have been known to follow the locusts in dense swarms, and I have myself seen them so thick that not a locust could rise from the ground without being pursued by several, and there is no escape from their pursuit until the persecuted victim gets high in the air.

4. *Instinctive Impulse.*—As already stated, the principal invading and return movements are aided by the prevailing winds. That they are not, however, solely dependent on the wind, but largely instinctive, is proved by numerous observations, showing that the insects in migrating pursue a definite course and will move only when the wind favors that course. This is particularly true of the return migration. The fact that there is a well-marked limit line, coupled with this return migration, is not to the same extent susceptible of any other explanation. The preservation of the species—an instinctive desire to get to a more congenial soil and climate—is evidently the principal inciting cause to this return migration. Hunger can have no part in it since the insects pass over great stretches of luxuriant vegetation, both wild and cultivated. The more I study these movements, the more I feel that, for their proper explanation, we must allow a certain amount of instinctive guidance, akin to that possessed by migratory birds.

## The Eastern Limit.

Nothing seems more natural at first blush than that an insect which is able to travel in the course of a single summer from the Northwest Territory to Galveston or points even farther south, and from the Rocky Mountains to central Iowa, should also be capable of extending to any part of the country. Yet the history of all past invasions, so far as we have any record of them, shows that there is a well defined limit beyond which the species has never reached to do harm, and beyond which there is every valid reason to believe that it never will, so long as the present configu-

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ration and climatic conditions of the continent obtain. The close correspondence between the eastern limit reached during the several invasions, since the year 1866, is truly remarkable, and the limit-line runs in a broad way along the 94th meridian. To particularize, it extends from a point starting from the southern end of Lake Winnipeg to some twenty miles east of St. Paul; thence south to Storey County, Iowa; thence westwardly, receding to northwest Missouri; thence bulging again to Pettis County, Mo.; and thence pretty direct through the extreme northwest corner of Arkansas, to Galveston, Texas. We have here a well defined limit line to the eastward spread of this destructive insect, more effectual than the highest mountain ranges in other parts of the country; and it may well be asked what is the explanation of this eastern boundary line. The fact is established, and there is much satisfaction in it for the farmers of the fertile country east of the line; but the explanation of the fact is another matter. Limit to the insect's power of flight was Walsh's explanation, but is unsatisfactory, since, *a priori*, there can be no such definite limit to the flight of an insect that may be carried by wind 1400 miles out at sea (as we know to have been the case with another species), and that can halt and rest at will. The reasons are, beyond much doubt, physical, and principally atmospheric. In the prevailing direction of the winds during the seasons of flight; in the increasing humidity consectaneous with decreasing altitude, and especially in the shade and moisture that accompany forest growth — we have more tangible reasons. The insect is a denizen of high, dry plateaus, thrives best in dry seasons and is injuriously affected by excessive moisture; and those familiar with the distribution of timber on our continent will observe that the limit line, as I first pointed out a year ago, follows very closely "the separation of the timber from the plains and prairie regions, or, more correctly speaking, the line that separates that vast region between the Mississippi and the mountains, in which the timber averages not more than six or seven out of every hundred acres, and that in which it averages twenty-five or thirty out of every one hundred" (The Locust Plague in the U. S., p. 67).

So in the Saskatchewan country the timber line marks essentially the limit of the species' northern spread; while toward the Pacific the moisture beyond the Cascade and Blue Mountain ranges appears to be the most obvious barrier to the westward spread.

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We thus get three important generalizations or rules for guidance in the solution of this locust problem. There is the invasion from the Permanent to the Temporary Region; the return migration from the latter to the former and the eastern limit of extension. There is yet a fourth rule which also has some bearing on the movements of this locust. It is that in the Temporary Region whenever the young insects abound to such an extent as to keep the earth bare of vegetation in summer (implying thick and extensive egg-laying the previous year), there will be no eggs laid in such devastated region the same year. This means that a year of utter locust devastation, like that of 1875 in many parts of the West, will be followed by one of perfect immunity from the pests.

Thus within a single year we have been able to map definitely the geographical range of the species, and to trace the source of the more injurious swarms. In short, what was mystery before is mystery no longer. Aside from a large number of new facts of scientific interest as to the insect's habits, development, anatomy, and enemies, we have learned how to cope effectually with the unfledged insects; while we point the way to practically prevent the disastrous incursions from the Permanent Region. With these economic details it would ill become me to detain this Association. When we reflect on the extent of territory involved, embracing fully one-half of the superficial area of the U. S., and remember how little that is definite is yet known of the source, range and movements of the *Œdipoda migratoria*, which has, since biblical times, so sorely devastated large parts of Europe and Asia; we may well feel some pride in our Government investigation. The importance of the work will best appear by the fact that the most careful estimates place the minimum loss to the western States and Territories, since 1873, at two hundred million dollars, to say nothing of indirect loss and suffering on the part of a frontier population ill-prepared to bear it. By virtue of the individual observations of the commissioners, and of the special assistants and correspondents employed over the country affected, they were able to state confidently in advance to the farmers of the West, that there would be no serious locust injury of a general character the present year, nor for some years to come; and we point to an emigration this spring to the locust devastated regions of Texas, Kansas, Iowa, Nebraska and Minnesota, unprecedented in the history of our country, as partly a result of this encouraging an-

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nouncement. There is much yet to be done in more fully studying portions of the Permanent Region yet unexplored; in giving practical application to the information obtained; and in securing the proper state coöperation and legislation to carry out our recommendations and suggestions.

In conclusion, I cannot forbear remarking that, whatever of credit attaches to the favorable results that have grown out of the investigation into this locust problem, is in no small measure due to Dr. F. V. Hayden, without whose knowledge of the needs of the West — his forethought and energy, the investigation would scarcely have been ordered by Congress.

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## A New Source of Wealth to the United States. By Charles V. Riley, of Washington, D. C.

[ABSTRACT AND ARGUMENT.]

This paper is largely statistical, giving tables of exports and imports of raw and manufactured silks for the last half century, and bringing out clearly the steady growth of the silk manufacturing industry of the country, especially during the last decade, under the protective import duties on manufactured goods. The author starts out with the axiom that the establishment of any such wealth-producing industry as that of silk culture and manufacture, as an integral part of our productive resources, is well deserving the attention of the people and of legislators. Just as the American Philosophical Society, little more than a century since, gave great impetus to, and fairly established, the silk industry in Pennsylvania — established it so firmly that, had it not been for the Revolution, it would undoubtedly have continued to grow from that day on — so there is no reason why the American Association for the Advancement of Science should not be partly instrumental in reëstablishing that industry on a broader and more permanent basis. The author rapidly sketches the history of silk culture the world over, and especially in the United States, showing how from 1740 to 1790 hundreds of silk growers in the

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Southern, Middle and New England States, and especially about Mansfield, Connecticut, were successful, and how the industry has flourished under the stimulus, at times, of State aid. He traces the causes of failure in the past, and the point is strongly brought out that they were all transient, not permanent ones.

Speaking of silk culture, Peter Delabogie, in an address published in the very first volume of the Agricultural Transactions of the State of New York (1801), said:

"GENTLEMEN: You have in your hands all the means requisite for success and for enriching yourselves with the culture of silk. It remains with you to compare and judge your many attempts in it, and discover wherein they have been defective."

Mr. Riley points out wherein the first attempts have been defective, and have consequently failed. Every writer, who has given the subject thorough consideration, has been of the opinion that there are no absolute obstacles in the way of silk culture in this country.

Mr. A. T. Lillie, in an admirable review of the silk industry in the United States, from 1766 to 1874, drawn up largely from personal reminiscences, says: "The production of raw silk in this country, which, in 1828, amounted in Mansfield alone to 3200 pounds, and which ceased entirely in 1844, has since been revived in California, meeting there with modified success. There is no good reason why it should not be again extended throughout the United States. Since the introduction of the mulberry seed by Dr. Aspinwall in 1766, the history of this culture has been pregnant with encouragement, and only a fortuitous conjunction of misfortunes checked it."

*Review of the attempts at Silk Culture during the past decade.*—Speaking of the attempts of M. Provost in California, which is conceded to be by climate eminently adapted to the growth of the mulberry, and the rearing of the worms, Mr. Riley attributes much of the want of permanent success, which followed the efforts made at San Bernadino, to the extravagant statements and excessive enthusiasm, verging on fanaticism, which characterized all of M. Provost's writings and utterances. "Had he been as prone to report failure as he was to magnify success, there would not have been a re-active depression, which was as unnatural as was the over-enthusiasm." M. Provost's little work, "The California Silk-Grower's Manual," was better calculated to induce another

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multicaulis fever than healthily to stimulate silk industry. "Its exaggerated pictures and immoderate statements annul what little value it did possess, and earn for its author the name of *blagueur*, which has been applied to him in France."[1]

Speaking of the effort of M. E. V. Boissiere, at Silkville, in Franklin County, Kansas, Mr. Riley expresses the highest appreciation of the intelligent attempts of the founder of that silk colony. The industry has, however, lagged, for the simple reason that it has been found less profitable than stock-raising and other general farming operations. M. Crozier, who has had charge of the silk establishment lately, has also found that the sale of eggs and the sale of mulberry cuttings are more profitable than the reeling and manufacture of the silk, and there is great danger that, from self-interest, greater harm than good will eventually come from this Kansas attempt. M. Crozier is now making every effort to encourage the purchase of his mulberry trees, an attempt which must be as injurious as it is premature, because it is stimulating an unnatural demand for the trees, in face of the fact—shown by his own experience—that there is no profit yet to be derived from the production of the cocoons.

Very much the same may be said of other efforts that have been highly applauded without sufficient cause, and of plans prematurely and glowingly reported. As an instance Mr. Riley mentions the accounts that have recently been published of the success attending the efforts of Mr. S. Lowery, of Huntsville, Alabama, who is reported to have successfully raised and spun and utilized the silk, and who is said to contemplate establishing an industrial academy for silk culture. Investigation shows that the few cocoons (reared by one of Mr. Lowery's daughters) are of an inferior Syrian race, having no commercial value; that the worms were fed on lettuce, and that the silk, which was carded and spun, is simply interesting as a curiosity, but not as an article of commerce.

*The present prospects of Silk Culture, and how to establish it.*—The paper points out the present prospects of silk culture and the way in which it may be established. In his report for 1877 to the President of the United States, the Commissioner of Agriculture gives tabular estimates of the value of many different products which we now import from foreign countries, and which may be produced at home, and this shows that over $236,000,000, not in-

[1] 4th Mo. Ent. Rep.

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cluding additional expense for freight and commissions, are paid annually for imports, all of which could and should be saved to the people. The item of silk alone foots up to over $23,000,000.

In his commendable efforts to increase the productive capacity of the country by the encouragement of neglected industries, silk culture has not been overlooked by commissioner Le Duc, and Mr. Riley expresses his gratification that his connection with the Department will enable him to do something for the permanent establishment of this industry, which he has followed with interest for many years. Mr. Riley shows that the time is rife for systematic, intelligent effort in the line of silk raising. The old argument, that we cannot compete with the cheap labor of Europe and Asia, no longer has the force it had in years gone by. With a large tramp element that is proving disastrous and annoying; with a large portion of the population in the more crowded Eastern cities out of employment; with any number of females and young people most anxious to earn something for a livelihood; with the people of the south looking for new avenues of industry — the cheap labor argument can no longer be successfully made against silk culture. The cheap labor of foreign countries is just as available for cotton and other industries as for silk, and the same objection has at one time or another been urged against most of our more important industries. Our people are neither lacking in industry, ingenuity nor energy, and will more than make up in respects for the more ignorant, though cheaper, labor of other countries.

But it is not a question, Mr. Riley argues, of whether we can compete with foreigners, either in living as cheaply or producing as cheaply. It is a question of adding to our own productive resources. There are hundreds of thousands of families in the United States to-day who would be most willing to add a few dollars to their annual income by giving light and easy employment, for a few months each year, to the more aged, to the young, and especially to the ladies of the family, who often have no other means of profitably employing their time.

This holds especially true of the people of the southern States, most of which are preëminently adapted to silk culture. The girls of the farm, who devote a little time each year to the raising of cocoons, may not earn as much as their brothers in the field, but they may earn something, and that something represents an

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increase of income, because it provides labor to those members of society who, at present, too often have none that is remunerative. Further, the raising of a few pounds of cocoons each year does not, and need not, materially interfere with the household and other duties that now engage their time, and it is by each household raising a few pounds of cocoons, that silk culture must in the end be carried on in this, as it has always been in other countries. Large rearing establishments seldom pay.

The white mulberry is already grown extensively in orchards in parts of North Carolina and elsewhere, simply for the fruit as food for hogs, while some persons have even found that it paid to raise silk, though the cocoons had to be shipped to Marseilles, France. Several bales of cocoons have been sent by Mr. E. Fasnach from Raleigh, N. C., to that port for about $3.00 freight per 100 lbs. The bales, 6 × 5 feet in size, averaged about 40 lbs. of stifled cocoons, and brought, in 1876, $2.50 per lb. A sufficient number of cocoons has lately been raised in this country to have warranted brokers in New York in offering from $1.75 to $2.00 per lb. for them.

Experiments that have been made in the past, and a series the author himself has been carrying on for the last ten years, abundantly establish the fact that the climate of the larger portion of the United States is admirably adapted to this culture. This is not only proved by the healthfulness of the worms, when intelligently fed and cared for, but by the fact that we have a larger number of native silk-producing insects than any other country of the same extent, and by the further fact, as proved by experience, that silk raised here is of a superior quality.

Now, experience shows that silk culture in the past has failed largely because of the want of a market, and Mr. Riley maintains that the first thing requisite is not the dissemination of mulberry trees or of silk-worm eggs, but the establishment of filatures or reeling factories. All attempts to stimulate the industry unduly are hurtful. It must grow, as all other industries have grown, and when once firmly established, on however small a scale, there can be no doubt that it will grow as steadily as all other industries have grown when once established in this country. The difficulties met with in the past have not been greater than they were in France and other countries, which persevered until failure after failure was overcome.

The great question is, how shall we encourage the production of

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silk and the reeling thereof? There is at present no market in this country for cocoons, except for those which are pierced, and which, having to be carded and spun, bring a very low price. Nor will there be a market for them until some one shall engage in the business of reeling. Yet it would not be prudent for any business man to engage in reeling until he had some assurance of getting a supply of cocoons. Now, how to break out of this close circuit is the problem. Just as the manufacturing industry has been stimulated by the encouragement given it by the National Congress, so silk culture should be in the same manner stimulated.

There ought to be no real antagonism between the manufacturer and the producer, and our manufacturers would certainly very gladly purchase home-raised silk in preference to that raised abroad, more particularly as they find it very difficult to prevent fraud and deception on the part of their Asiatic dealers. Yet a duty on raw silk, that would encourage or stimulate the growth thereof in this country, would naturally meet with the opposition of manufacturers, and here lies the whole difficulty. Yet it is not an insuperable one. Protection of a productive industry in its infancy adds to the general wealth; protection of the manufacturing industry, while desirable as a fostering agency, becomes in time injurious to the masses by benefiting the few and encouraging monopoly, unless wisely gauged and enacted. There is always great danger of unwise legislation in the interest of manufactures, brought about by influences which associated capital can always wield.

With improved machinery, with superior labor, and the greater profits that naturally flow from them, our silk manufacturers have prospered, and their late rapid increase during a period of general depression in other branches of industry, is the best evidence of the fact. They are in a position to bear a small import duty on raw silk. If Congress will either impose such a duty, or offer a bounty on home-produced silk, and in addition give the Department of Agriculture the means to erect proper reeling machinery, and to introduce properly trained reelers, so as to form a nucleus for the reeling branch of the industry, this is all, in Mr. Riley's opinion, just now needed from the National Legislature. The dissemination of proper information and of silk-worm eggs, may be left to the Department.

Our attempt should be to encourage the culture of silk, and the reeling of silk, without, if possible, affecting or injuring the

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throwing and twisting business, which is perhaps not firmly enough established to be able to continue without some protection. To cripple or destroy or impair this industry would, of course, remove the market of the silk-grower, and there is no absolute necessity for any present change in the present tariff on manufactured silks.

Mr. Riley believes further that each state should encourage not only the culture of silk, but offer a bounty of, say 50 cents or $1 per pound for the choked cocoons, and $1 or $1.50 per pound for reeled silk.

*Silk and the Osage Orange.*—The part of the paper that is, perhaps, the most interesting from a scientific point, is that relating to the production of silk from the Osage orange.

Mr. Riley has been very much interested in this part of the inquiry for many years, and has raised consecutively on *Maclura* for the last eight years a race or breed of worms—a cross between one of the best Japanese and one of the best French races—with great success. He found that the worms became, if anything, more and more healthful and hardy upon this plant. Silk, from Osage orange-reared worms, when fed intelligently, was in both quality and quantity equal to that from mulberry-fed worms. This important fact is not admitted by those who have mulberry trees for sale, but Mr. Riley has good reason, based on experience and experiment, for the statement. This he considers a very strong point in favor of the United States, as, in case the mulberry trees should at times be cut off, as they were at the close of the multicaulis fever of 1839 and 1840, by mildew and disease, American silk-growers would, nevertheless, continue their calling by employing the Osage orange, which, throughout that portion of the country best adapted to silk culture, thrives, and is never subject to any such injury. Moreover, in order to commence the raising of silk, the average farmer will not have to wait for the growth of the mulberry trees, or lay out any capital therefor.

There is an increasing interest manifested throughout the country on this subject. Inquiries are constantly being received at the Department of Agriculture as to how to attend to the worms, and on all possible subjects connected with silk culture, and it is the intention of the Department to publish in due time, and distribute a comprehensive manual of instructions, which, by widely disseminating the desired information on the subject, will help to establish the industry.

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## NOTES ON THE LIFE-HISTORY OF THE BLISTER-BEETLES AND ON THE STRUCTURE AND DEVELOPMENT OF HORNIA. By CHARLES V. RILEY, of Washington, D. C.

[ABSTRACT.]

AT the Hartford (1874) meeting of the Association, Mr. Riley described the newly-hatched larva of some of our common blister-beetles; but all attempts to trace their habits had proved futile both in this and other countries until 1877, when he discovered that they preyed on the eggs of locusts (*Acrididæ*). In a paper published in the last volume of the "Transactions of the Academy of Sciences," of St. Louis, the life-history of several of our common blister-beetles is traced. The present paper gives a brief résumé of the facts there recorded, showing that the beetles belonging to the genera *Epicauta* and *Macrobasis* go through the same curious hypermetamorphoses as do other species of the family *Meloïdæ*, and especially as *Meloë* and *Sitaris* were already known to do. The larva hatches as an active, pale-brown, long-legged creature, termed triungulin on account of its three-clawed tarsus. It then changes to what Mr. Riley calls the *Carabidoid* stage, in which it is white, less active and fleshy; then to what he calls the *Scarabæidoid* stage, in which it is still more degraded and clumsy; then hardens to what he calls the *Coarctate larval* stage, in which it is perfectly helpless and resembles the *puparium* of many Diptera; then to the final larval stage, in which it is again white and soft and more or less active; then to the true pupa state; and, finally, to the beetle; existing, thus, in eight distinct states (including the egg), instead of the four in which ordinary insects occur.

The paper is principally devoted, however, to the life-history of a very anomalous, wingless genus of this family, the *Hornia minutipennis* Riley. This insect is degraded and subterranean, and was found in the cells of a common mason bee, the *Anthophora abrupta*, Say. Its life-history, which was not known at the time the species was described, has been completely made out by Mr. Riley the present summer. The eggs, which are laid loosely in the burrows of the bee, hatch during the early part of June. The triungulin is extremely active, and, in all essential characters, very similar to that of *Sitaris*, one species of which, in Europe, likewise develops in the cells of *Anthophora*. By means of its

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tarsal claws and a pair of pre-anal spinnerets and claspers, it holds on very tenaciously to the hairs of the bee, and is carried on the same into the bee-burrow. When the bee-egg is laid, and before the cell is capped over, this triungulin disengages itself from the bee and at once pounces upon the bee-egg. After having sucked the contents of this last it throws off the triungulin skin and assumes the *carabidoid* stage; thereafter feeds upon the honey-paste stored by the bee, and, within the cell, goes through all the hypermetamorphoses characteristic of the family. All the later stages, however, take place within the puffed skin of the *scarabæidoid* larva, the *coarctate* larva skin being well separated therefrom, and the third or final larva skin, which is very delicate, as well as the pupa skin, being worked off posteriorly where they are found in a shrunken compressed mass. There is but one brood annually; the pupa state being attained in August, and the beetle maturing all its parts during the autumn, and lying within its coverings until the following May.

The paper contains some interesting details as to the effects of rain both on the bee larva and the *Hornia* larva; on the vicissitudes that befall the triungulin, its fratricidal propensities when two or more are inclosed in the same cell, and on its adaptability to varying amounts of food-supply.

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## On the Larval Characteristics of Corydalus and Chauliodes and on the Development of Corydalus cornutus. By C. V. Riley, of Washington, D. C.

[ABSTRACT.]

The paper relates to the development of one of the most singular and interesting of North American insects—the largest of the order Neuroptera. In its perfect state this insect is a great, clumsy, nocturnal fly, popularly called Hellgrammite, and characterized by the jaws of the male being converted into a pair of long, curved, cylindrical, and tapering prehensile organs, like the finger of a grain cradle. In the larva state it is aquatic and much

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esteemed as fish-bait by fishermen, who call it a "crawler," "dobson," etc. Indeed, one of the most popular artificial fish-baits is a patent india-rubber imitation of it. This larva is very peculiar in having in its later stages three distinct sets of breathing organs, viz.: the ordinary spiracles, a lateral series of long, single branchial filaments, and a ventral series of spongy branchiæ, composed of numerous branching and tractile filaments. The eggs of this insect are laid, to the number of about three thousand, in curious masses on the leaves and branches of trees, or upon any other object overhanging water, and were first described by Mr. Riley at the Buffalo (1876) meeting of the Association. After comparing the eggs with those in the female abdomen, and the newly hatched with the mature larva, he felt quite certain as to the parentage of the curious eggs. Yet the newly hatched larva which he described differed from the mature larva in lacking the ventral branchiæ, resembling in this respect the mature form of another aquatic larva of an allied genus (*Chauliodes*), and as some leading entomologists believed that the eggs described by Mr. Riley might belong to this last genus, further evidence as to the real nature of said eggs was desirable. The paper presents this evidence and confirms the previous determination. The *Corydalus* larva is traced through its stages of growth and then compared with that of *Chauliodes*. Several interesting scientific facts are brought out. The larva undergoes about six molts. The double nature of the thoracic tracheæ in *Corydalus* appears in the first larval stage, and the branchial nature of the lateral filaments is proved by the tracheæ leading to their tips. The ventral branchiæ first appear in the second stage (after first molt) and from three main stems each with bifurcate or trifurcate filaments. The branching filaments become more and more numerous and complex with each molt. The tracheæ also lead more and more strongly to these ventral branchiæ and less strongly to the lateral ones, with age. The stigmata are obsolete in the first three stages and in the fourth are only clearly distinguishable on the four or five larger abdominal joints, being still obsolete on the terminal ones.

The motion of the larva is invariably backwards. When newly hatched it moves actively about in the water by sudden sweeps of the abdomen beneath, very much as a lobster is known to do; and even, when full grown, a somewhat similar motion is employed in

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swimming. In the water a constant motion of the ventral branchial tufts is kept up, the main stem being first moved quickly backward and upward so as to bring the whole tuft close to the body, the filaments of which it is composed being then closely appressed to each other. The main stem is then brought more slowly down in the opposite direction, when the filaments spread and enlarge the whole to its utmost. In pure water the motion occurs about once a second; as the water becomes impure the motion becomes more rapid and the larva issues from the water as soon as possible, being able to live out of water for several days even when only two-thirds grown.

The paper gives detailed comparative descriptions of the *Corydalus* and the *Chauliodes* larvæ. This last may always be distinguished from the former by having a smooth and unarmed skin; that of *Corydalus* has a skin roughened with granulations and capitate or clavate projections (overlooked by previous describers), the little projections being visible even in the first stage, at which time they are less capitate. The *Chauliodes* larva has the last pair of spiracles on the tips of a pair of contractile filaments described as setæ by Walsh, who failed to apprehend their real nature and wrongly described the *Chauliodes* larva as having one pair of spiracles less and one abdominal joint less than that of *Corydalus*, whereas both larvæ have the same number of joints and spiracles and both possess the rudimentary mesothoracic spiracle, which Mr. Riley finds more common in insects than is generally supposed. In other structural respects, as well as in habits and transformations, the two larvæ greatly resemble each other. The eggs of *Chauliodes* have a longer tubercle or stem on the top, and are not covered with white albuminous material as are those of *Corydalus*. Mr. Riley has obtained large additional numbers of the egg masses of the latter the present summer, finding them not only on the leaves as described in his former paper, but on the stems of different trees, as well as on rocks overhanging water. He has had as many as twenty egg masses on a single maple leaf, both sides of the leaf being completely plastered up by them; and as a large number of these masses will generally be found in some one particular locality, or on a few branches of the same tree, the assumption is that the females congregate for purposes of oviposition. The white, albuminous substance covering these eggs shows by analysis that it has all the physical properties of wax.

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BIOLOGICAL NOTES ON THE GALL-MAKING PEMPHIGINÆ. By CHAS. V. RILEY, of Washington, D. C.

[ABSTRACT.]

THE life-history and agamic multiplication of the plant-lice (*Aphididæ*) have always excited the interest of entomologists and even of anatomists and embryologists not specially interested in insects. The life-history, however, of the gall-making species belonging to the *Pemphiginæ* has baffled the skill of observers more than that of any other group.

Mr. Riley is about to publish some new biological discoveries relating to this family of insects, in connection with a descriptive paper by Mr. J. Monell, of the St. Louis Botanic Gardens. The paper laid before the Association simply records some of the yet unpublished facts discovered.

All of the older writers in treating of the different gall-producing *Pemphiginæ* of Europe have invariably failed to trace the life-history of the different species after the winged females leave the galls, and, with few exceptions, have erroneously inferred that the direct issue from the winged females hibernates somewhere. The most recent production on the subject is a paper published the present year in Cassel, Germany, by Dr. H. F. Kessler, entitled the "Life-History of the Gall-Making Plant-Lice, affecting *Ulmus campestris*." The author, by a series of ingenious experiments, rightly came to the conclusion that the insects hibernate on the trunk, but he failed to discover in what condition they so hibernate.

Led by his previous investigations into the habits of the Grape Phylloxera, Mr. Riley discovered, in 1875, that some of our elm-feeding species of *Pemphiginæ* produce wingless and mouthless males and females, and that the female lays but one solitary impregnated egg. Continuing his observations, especially during the present summer, he has been able to trace the life-history of those species producing galls on our own elms, and to show that they all agree in this respect, and that the impregnated egg produced by the female is consigned to the sheltered portions of the trunk of the tree, and there hibernates—the issue from it being the stem-mother which founds the gall-inhabiting colony the ensuing spring.

Thus the analogy in the life-history of the *Pemphiginæ* and the

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*Phylloxeræ* is established, and the question as to what becomes of the winged insects after they leave the galls is no longer an open one. They instinctively seek the bark of the tree and there give birth to the sexual individuals, either directly, or through intervening generations.

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## On the Osteology of Sciuropterus volucella, Geoff. By George H. Perkins, of Burlington, Vt.

[ABSTRACT.]

The results of an examination of about a dozen specimens of the flying squirrel (*Sciuropterus volucella*, Geoff.) are given in the paper of which the following pages form an abstract.

In general it may be noticed that the cranium soon after birth is of a conical form, with large triangular fontanelles. As the age increases the form of the skull becomes less evidently conical, the cranial portion grows longer and the facial wider. In specimens not more than two or three months old the upper surface of the skull is strongly convex, but in adult specimens it is usually flat. In the former, the highest part of the skull is just behind the coronal suture, and from there the surface slopes regularly backwards and forwards, but in the latter the skull is highest in front of the coronal suture just back of the post-orbital processes. In young specimens the parietals form a not inconsiderable part of the lateral portion of the skull, while in adult specimens they form little or no part of this portion of the skull, and in young specimens each parietal is little longer than wide, but in adults it is very much longer. Similar changes take place in the form of the frontals. In the young these bones are gibbous between the coronal suture and the post-orbital processes, and depressed in front of the latter, especially at the nasal suture; but in adults there is little depression at this point and little elevation at the other, and the nasal end, at first much narrower than the other, becomes at length quite as wide. The nasals change less than either the parietals or frontals.



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As seen from below, the differences between old and young skulls are less obvious than when seen from above. In adult specimens the width across the zygomas is nearly two-thirds of the length of the skull, and the greatest height is about three-sevenths of the length, and these proportions were constant in all the specimens examined. Removing the zygomas we find that the circumference of the skull regularly increases from the front to the base, but the large size behind is due chiefly to the large auditory bullæ; leaving these out of account the circumference is greatest just in front of the glenoid fossæ. The base of the skull is oval and rather more than half as high as wide.

Bones of the Cranium. The *basi-occipital* unites quite early with the surrounding bones, but in young specimens it is seen to be tapering in front and longer than broad. It bears a distinct spine at the foramen and from this a crest runs forward until it blends with the surface. The *ex-* and *par-occipitals* are united at birth and soon after the *basi-* and *supra-occipitals* coalesce with them. The *ex-occipitals* form the whole of the condyles. The *supra-occipital* forms a large part of the base of the skull but does not extend over the upper part. The *basi-sphenoid*, *pre-sphenoid* and *pterygoids* are united very early, and the *vomer*, *ali-sphenoids*, *orbito-sphenoids* and *palatines* are distinct only in very young specimens. The *squamosals* are of very irregular form and occupy a large portion of the lateral part of the skull. The zygomatic process is broadly triangular and stands directly outwards at first, but soon turns forwards and downwards. It forms but a small portion of the zygoma not extending beyond the glenoid fossa. This fossa is rather deep, longer than broad, its outer margin is deep and formed chiefly by the malar. There is no glenoid process.

The *periotic*, owing to the large tympanic and mastoid bullæ, is large, thick, and in general, of oval outline. It is largely overlapped by the squamosals, to a less extent by the supra-occipital, which is very early united with it. In very young specimens the *tympanic* is separable as a delicate ring reaching about two-thirds around the meatus. The mastoid bulla is not large and is of irregular form, while the tympanic bulla is very large, and reniform in shape. The meatus is very large, of ovate outline externally and situated near the centre of the external wall of the periotic. The *parietals* are, in general outline, trapeziform, nearly as wide as

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long, and rather wider in front than behind. They are thin, delicate bones. They form very little of the lateral wall of the cranium, being chiefly confined to the upper part. The *sagittal suture* is indistinct, except in young specimens. In specimens less than three months old there is a distinct *interparietal*, of a broadly triangular form, emarginate behind and extending anteriorly about a third of the length of the sagittal suture. The *frontals*, viewed from above, are long, narrow bones which coalesce quite early. The upper surface is almost entirely flat. The lateral portion bends down almost at a right angle with the superior portion, to form a large part of the orbit. The anterior and posterior borders of this portion approach each other until, at the orbito-sphenoid, they meet in a short point which is wedged in between that bone and the maxillary. The *nasals* are long, narrow, increasing greatly in width anteriorly and extending somewhat beyond the premaxillaries. Together they are spatulate, strongly arched in front and of large size. The *premaxillaries* are very large, quadrangular, widest above, and occupy much of the space usually occupied by the maxillaries. Above they extend back to meet the frontals. Along the median line of the palatine portion runs the usual ridge, and on either side are the *anterior palatine foramina*, which are very long and narrow and terminate at the suture with the maxillaries. The *maxillaries*, being greatly modified by the great development of the premaxillaries, form no part of the superior portion of the cranium. The *zygomatic process* is very large, triangular, thin, and with a nearly flat anterior surface. The *malar* is nearly horital *in situ*, when detached it is shaped like an italic *f*. It is compressed laterally in the middle and is stout and forms the chief portion of the zygoma. The *pterygoids* are very delicate bones, and their short hamular processes reach back nearly to the auditory bullæ. The *palatines* and *vomer* present no peculiar features; the wing-like processes of the latter extend upwards and backwards and are half as long as the entire bone. They are thin and flat, and bear at the end delicate transverse processes. The *ethmo-turbinal* is also a very delicate bone shaped much like the sternum of a passerine bird. The *mesethmoid* is a quadrant-shaped lamina extending for more than half its length beyond the *turbinals*. These latter are pyramidal in form and largely developed. The *mandible* is compact, short, and in very young specimens, shows three parts. The rami unite by simple chondrosis without sutural

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interlocking. The *coronoid process* is a sharp, slender point, reaching somewhat above the condyle. The *pterygoid* and *masseteric fossæ* are large and deep. The *angular process* is wide, deep and somewhat incurved below. In young specimens the rami are relatively longer, narrower and more divergent than in the adult and have less oblique coronoids, narrower angular processes and shallower fossæ. As the growth of the condylar portion proceeds, the increase is chiefly in height. The teeth present the usual characters of the Sciuridæ.

The Vertebral Column. — *Cervical Vertebræ.* These are firmly interlocked and allow little motion except in a vertical plane. The *atlas* is very large and strong, greatly exceeding in size any of the rest. The condylar surfaces are oblong, oval, oblique, the outer border much elevated and together they form a deep cup-like cavity in which the head rests securely, though with considerable freedom of motion. The vertebrarterial foramen is large. There is also a large perforation for the exit of the first spinal nerve. The *axis* is chiefly noticeable for its enormous spinous process, which projects both anteriorly and posteriorly over the neural arch. The remaining five vertebræ are quite similar; in each the neural canal grows broader and lower than it is in that in front; the body remains with little change, the transverse processes, which in the atlas are mere tubercles, grow longer as we go towards the last. The neural spine suddenly becomes small back of the axis and gradually disappears at the seventh vertebra. The pre- and post-zygapophyses are prominent. All the transverse processes, except those of the first and seventh vertebræ, bear each an inferior lamella, which, in the second, third and fourth, is a thin plate, but in the fifth and sixth becomes much larger and somewhat claw-shaped.

*Dorsal Vertebræ.* As the seventh cervical presents many of the characters of the dorsal vertebræ, the transition from one group to the other is not abrupt. Taken together the twelve dorsals present a sigmoid curve with greatest freedom of motion in the anterior five or six. When the back is hunched, as it is in the ordinary position of the animal as it eats, holding its food in its paws, the chief motion is at the junction of the last cervical and first dorsal and last dorsal and first lumbar, two or three of the adjacent vertebræ in each case partaking more or less in the movement. In the first dorsal the transverse processes become much

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larger and are no longer pointed, but truncate at the ends. The neural arch bears a short, lamellar spine in the first dorsal, which is much larger in the second, and after this becomes longer and inclined more and more obliquely until the eighth is reached, when a change occurs and in the ninth it is shorter and more vertical. All preceding the ninth have the transverse processes and post-zygapophyses forming a common projection, bifid at the tip, but in the ninth the transverse processes are long, thin laminæ, T-shaped at the end, and in the tenth they are stout and bent backward so as to embrace the pre-zygapophyses of the eleventh. This and the twelfth are much larger than any that precede them. The neural arch of the dorsals increases in width until, in the tenth, it is three times as wide as in the first. It does not increase gradually, but irregularly, being of the same width in several, then suddenly growing larger, and then in two or three vertebræ remaining unchanged, and then again growing larger and so on.

The seven *lumbar vertebræ* form a strongly interlocked series, bearing each a thin spine, which is largest on the sixth and seventh. There are anapophyses upon all except the last two, but none bear hypopophyses. There are well developed metapophyses, these being stouter than the transverse processes. The bodies of all are strongly convex laterally above and below and concave longitudinally.

The *sacrum* is a long, narrow bone made up of three anchylosed vertebræ, the first being nearly twice as large as the other two. The neural spines are long, upright and conspicuous. The first sacral vertebra is broader than any other in the whole series, and its transverse processes bear large synchondrosial surfaces, to which the innominate bones are wholly attached, but the transverse processes of the second are confluent with this, and those of the third with the second. The neural canal is abruptly contracted in passing from the dorsal to the sacral vertebræ.

There are twenty *caudal vertebræ*. The first four are alike in form and size, but with the fifth the body begins to elongate and the processes to diminish, and at the eighth the latter have nearly disappeared. The neural canal disappears at the sixth. The bodies increase as far as the ninth and in this and the next two are of nearly equal size. After the eleventh the body grows shorter and more slender until the last is reduced to a mere rod of the same length as the first. From the sixth to the twelfth the

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processes gradually disappear and are almost wholly obliterated at the twelfth.

There are *chevron bones* between each pair of vertebræ from the third to the fourteenth. They vary in form and size — the first being slender and V-shaped, while those that follow are less and less so, the sides becoming more and more parallel with an oval aperture. In all there are eleven. The third is largest.

Bones of the Trunk. The *thorax* is of a conical form, its antero-posterior diameter is much less than its transverse. There are twelve pairs of ribs seven of which are attached directly to the sternum, the other five to each other; the attachment of the last two is more slight than that of the rest, but none can rightly be called free. The *sternal ribs* are all more or less ossified. The *vertebral ribs* increase in length to the seventh, beyond which they decrease, while the curvature is greatest at the fifth.

The *sternum* consists of six parts: a broadly triangular *presternum*, a *mesosternum* of four linear bones, and a linear *xiphisternal* bone to which is added a spatulate *xiphoid* cartilage, which is thin and flat.

Bones of the Fore Limbs. The *scapula* is triangular. The coracoid border is much shorter than the glenoid. The post-scapular fossa is very deep and narrow, while the pre-scapular fossa is deltoid, wider and shallower. From the acromion to the supra-scapular border runs a high, thin spine. The acromion is large and projects over the glenoid, from which it is separated by a deep notch which appears like a continuation of the post-scapular fossa. The coracoid is prominent, little curved, bends obliquely inwards and downwards. The glenoid cavity is shaped like the bowl of a spoon and is narrowest above. The *clavicle* is a compressed, nearly straight bone, thinnest and widest at the acromial end and somewhat dilated at the sternal. The *humerus* is stout, rather long, bent slightly at each end with the main shaft straight. The head is large and without a neck. The bicipital groove is very slightly developed, nor are the tuberosities large. The deltoid ridge is high and sharp, but extends only about one-third of the length of the bone. The supinator ridge is much longer than the deltoid and winds around the shaft from the condyle, making about half a turn, disappearing a little below the middle of the posterior side of the bone. A supra-trochlear foramen is present in some specimens and not in others. The *ulna* is very slender, much longer than the radius,

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with a very large olecranon. Above the coronoid the bone is flattened laterally and is thick and heavy. The olecranon is very large, its inner surface flat, but the outer bevelled so that a thin crest runs obliquely along the top and down to a point opposite the sigmoid excavation. Its general position is oblique with reference to the shaft, and the coronoid projecting obliquely inwards and the olecranon outwards the sigmoid is very oblique. The *radius* is shorter than the ulna and much stouter. The lower portion of the shaft is nearly straight; a little above the middle it is bent forwards, but above the tubercle it is again straight. In supination the lower part lies parallel with the ulna, while in pronation the bones cross, though not to a very great extent.

There are seven *carpal bones*, the scaphoid and lunare being united. The wrist is strongly arched laterally. The *metacarpals* form two sets; the first and fourth are stout and about equal, as are the longer and more slender second and third. The proximal *phalanges* are somewhat curved, while the middle are nearly straight, and these present the same arrangement in pairs as that noticed in the metacarpals. The ungual *phalanges* are of nearly equal size. Between each metacarpal and proximal phalanx are two oval *sesamoids*. The *pollex* is wanting.

Bones of the hind Limb. The union of the *ilium*, *ischium* and *pubis* occurs within a day or two after birth. Behind the acetabulum each *innominate* bone is triangular, and the ilium projects forward as a long, slender bone. The acetabular surface is nearly horizontal and about on a line with the margin of the acetabulum. It is strongly bevelled to the pubic border, which is quite regularly straight. The ischial border is slightly curved along the sacral surface. The supra-iliac border is thin and oblique where it bounds the iliac surfaces. The pubic portion is formed by a slender, linear bone flattened vertically, growing wider behind. The symphyses are small, oblong. The thyroid foramen is very large, ovate, widest behind. The *femur* is a long, strong bone tapering but slightly towards the proximal end. The shaft is straight, flattened transversely. The head is large, neck short, and both are strongly inclined. There is a greater, lesser and third trochanter, the first being very large. The whole surface is smooth and presents no evident linea aspera. The *patella* is thick, quadrilateral in form. The *tibia* is a much curved, long, stout bone, nearly as thick as the femur. It articulates alone at the knee, the fibula

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taking no part in this. This bone is unusually developed in the species under consideration. The shaft has a well marked sigmoid curvature. The *fibula* is very long and exceedingly slender, and its lower third is firmly anchylosed with the tibia. The shaft is much compressed and twisted upon itself. There are eight *tarsal bones*. The *metatarsals* are of the usual long, slender form; the fifth, owing to its backward prolongation, is longest, and from this each is shorter than that next outside. The proximal ends are much dilated. The proximal end of the fifth is continued beyond the rest and forms a cylindrical, diverging process on the outside of the tarsus and, instead of articulating at the end as the rest do, it bears facets on the inner side by which it articulates with the fourth metatarsal and slightly with the cuboid, and at this point the shaft is dilated and bears a triangular tubercle, and farther on towards the distal end are two smaller tubercles on the outside. The *phalanges* and *sesamoids* present no unusual features. There is a large *os penis* which is somewhat twisted upon itself, dilated at each end and terminated by a bifid knob resembling a condyle.

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NOTES ON THE DEVELOPMENT OF AMIA. By S. A. FORBES, of Normal, Ill.

[ABSTRACT.]

THIS paper is essentially a partial description of the form and structure of specimens of Amia from 20 to 30 mm. long, taken in the Illinois river in June, 1878.

The smallest specimens have a median fin, undifferentiated except for a trace of the true caudal, commencing immediately behind the head and terminating about the middle of the belly. This fin is continuous except at the vent, is stiffened throughout by primordial filaments, and forms posteriorly a lanceolate tail like that in the smallest gars. The notochord is continued into it without flexure.

The pectoral fins are distinctly lobate in external form. Their skeletal structure is not yet clearly made out in its details. A

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basal, cartilaginous piece, and very soft and delicate radial pieces exist, but their number and relations have not yet been positively settled. There is no appearance of true fin rays. In a larger specimen (57 mm. long) it is shown that the radialia are all attached to the border of the metapterygium, none of them reaching the scapular arch, and that the upper fin ray does not embrace the mesopterygium except at its tip, and consequently does not reach the coraco-scapular cartilage. In specimens 25 mm. long a few rays are developing in the upper half of the pectoral. The ventral fins of these specimens are as yet rudimentary, presenting a mass of forming cartilage within the base, which sends three slight processes forward, apparently corresponding to the three basal pieces of the fin of the sturgeons.

Unexplained structures imbedded in the epidermis of the head are clusters of slender flask-shaped cells set perpendicular to the surface, the smaller ends projecting slightly through circular openings in the epidermis.

The gular plate consists of a fold of the skin homologous with a similar fold in the young gar, but thickened somewhat by a deposit of connective tissue cells and commencing to ossify by a single centre. No trace of composite structure is visible.

The bulbus arteriosus was found to contain already two well-developed pairs of valves, while its walls were provided with an abundance of muscular tissue.

Light is thrown upon the origin of the spiral valve in Amia by the fact that in these specimens the posterior intestine composes a close spiral coil of six turns, the interior of the intestine itself being simple.[1] A slightly cellular air-bladder is already developed.

The condition of the brain is important as furnishing some evidence of relations with Polypterus, and affording a hint with reference to the homologies of the four anterior lobes. The medulla oblongata is incomplete, the walls of the fourth ventricle being low and not incurved. The cerebellum is rudimentary, consisting only of a transverse plate supporting the posterior half of the optic lobes. The latter and the olfactory lobes are the most fully developed parts of the brain. The conarium is very large, nearly filling the transverse fissure (Gehirnschlitz of Gegenbauer) and the infundibulum is broadly open beneath. The optic thalamus is prominent, the lobi inferiores large, and the transverse fissure

[1] I am not sure of this, as no sections were made.

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wide. The hemispheres are flattened kidney-shaped, thick and convex above, thin and straight below, and connected by a commissure on the posterior half of their inferior border. The eversion of the upper border is indicated by a slight longitudinal groove. The rather wide space between the two hemispheres is arched over by a thick membrane which sends a longitudinal fold downward, dividing this space into two lateral ventricles of which the hemispheres form the thickened outer walls. From these ventricles, large foramina open into the olfactory lobes. This condition of things implies that the hemispheres of most fishes correspond most closely, as Tiedeman supposed, to the *corpora striata* of mammals. In cases where the foramina open behind the line of division between cerebral and olfactory lobes, it may be supposed that the absorption of the inner walls of the lateral ventricles stopped short of completeness.

Studies of the embryonic brain will apparently be needed to decide these dlsputed points.

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## NOTE ON THE DISCOVERY OF A HUMAN SKULL IN THE DRIFT NEAR DENVER, COLORADO. By THOMAS BELT, of London.[1]

[ABSTRACT.]

IN passing westward from the Missouri to the Rocky Mountains through Nebraska, northern Kansas, Wyoming, and Colorado the upper member of the drift series, the equivalent of the brown clay

[1] It is with deep regret that I record the sudden death of the talented author of this paper, which occurred at Kansas City, Mo., on Sept. 28, a month after the adjournment of the St. Louis Meeting. Mr. Belt was elected a member of the Association at St. Louis, and was present for two or three days when he received a dispatch recalling him to Colorado. During Mr. Belt's short stay in St. Louis, his courteous and genial manner won the respect of all who were so fortunate as to meet him. On leaving St. Louis he addressed me as follows:—

LINDELL HOTEL, ST. LOUIS, AUGUST 25, 1878.

PROF. F. W. PUTNAM. -*My Dear Sir:* An urgent business telegram starts me back for the West early to-morrow morning; and I shall, much to my regret, be unable to attend any more of the meetings of the Association. I have been so inconvenienced by this sudden change in my arrangements, that I have only had time to-day to draw up a very short account of the discovery of the human skull, very little more than the abstract I gave you yesterday, and have had to omit altogether the discussion of the age of the deposit in which the skull was found. I hope you will forgive this as it has been unavoidable. Thanking you for the kindness and courtesy extended to me, and hoping that I may be able to say good-bye to you personally, before I leave, I am Dear Sir, Yours very truly, THOMAS BELT.

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of Illinois, is found to be continuous up to the base of the mountains. Drift gravel is generally found below it or is contained in patches in it. Nearing the mountains the clay is of a darker color and a more sandy texture, and has often pebbles and stones scattered through it. Large boulders are also met with near the mountains and beds of sub-angular boulders and pebbles, nearly always of crystalline rocks, skirt the flanks of the mountains, run up the sides of the valleys opening into them and cap isolated hills in front of them to heights of about 8000 feet above the sea.

In the neighborhood of Denver the sandy clay covers the whole country excepting on the steeper slopes, where it is sometimes absent from denudation. It is often as much as thirty feet in thickness, and contains here and there boulders of crystalline rocks scattered through it. In some of the sections examined by me these were sufficiently numerous to make it a boulder-clay. Much of it is unstratified and with vertical joints.

Whilst examining the sides of a cutting through this deposit on the Colorado Central Railway, near the top of the low watershed between the South Platte and its tributary, Clear Creek, I noticed a whitish substance on the face of the cutting. On clipping round it with a knife it proved to be the top of a human skull.[2] It was in perfectly undisturbed loess-like clay, at a depth of three feet, nine inches, from the surface. At the same horizon at intervals were small angular pieces of miocene wood. No other bones were found and none of the teeth. Seventy yards distant I found what appears to be a portion of a human rib.

Isolated bones of other mammals than man are not uncommon in the neighborhood of Denver, and near the foot-hills in the brown clay and associated drift gravels. Besides those found by myself, Prof. Lakes, of Golden, and Mr. H. C. Towner, of Denver, have collected many specimens. Amongst these, bones of the common buffalo seem to be most frequently met with.

[2] The several fragments of the human cranium were seen by myself and other members of the Association at St. Louis. — F. W. PUTNAM.

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## Description of a Cliff-House on the Mancos River of Colorado, with a Ground Plan. By Wm. Fellowes Morgan, of New York.

Having recently visited a cliff-house on the Mancos river in Colorado, I offer the following description and ground plan of the house for the consideration of the section.

The Mancos river rises in the La Plata mountains in the southwestern part of Colorado, and after flowing in a southwesterly direction it falls into the San Juan river in New Mexico, very nearly at the corner of one state and three territories. It should be noticed that the high lands bordering on the Mancos river belong to the mesa, or table land, formation and to what is known as the *mesa verde*.

The valley is entered from the east through Thompson's park, one of the prettiest in the Rocky mountain region. This entrance is near the base of the La Plata mountains. The valley of the Mancos at its upper end is from a half to three-quarters of a mile wide, and partly under cultivation by irrigation, having been lately settled by a few American farmers. We went down the valley eight or ten miles to the ranch of John Gregor, and then twelve miles to the cliff-house. Some eight miles below John Gregor's the valley, which for several miles has been enclosed by cañon-like walls, becomes a cañon about 300 feet wide from wall to wall. There is one trail from the upper end of the cañon which leads past the cliff-house, and this is the ranchmen's trail which runs down the western side of the river.

Near the cliff-house the trail is about thirty or forty feet above the river, and the cliff-house is situated about two or three hundred feet above the trail. The cliffs are composed of a sandstone which, originally light, darkens by exposure. Here and there the cañon is deeply eroded at a great height from the present level of the river, leaving large cavernous openings in the face of the rock. It is in these openings that the cliff-houses are to be found. The great cavity in the rock, formed by natural causes and in which the cliff-house is situated, can easily be seen from the trail, but of the front wall of the house only a very small part can be seen, and that only by one who knows where to look for it. In this case the upper line of the natural excavation was a great arch which tended to prevent the roof from falling. The depth of

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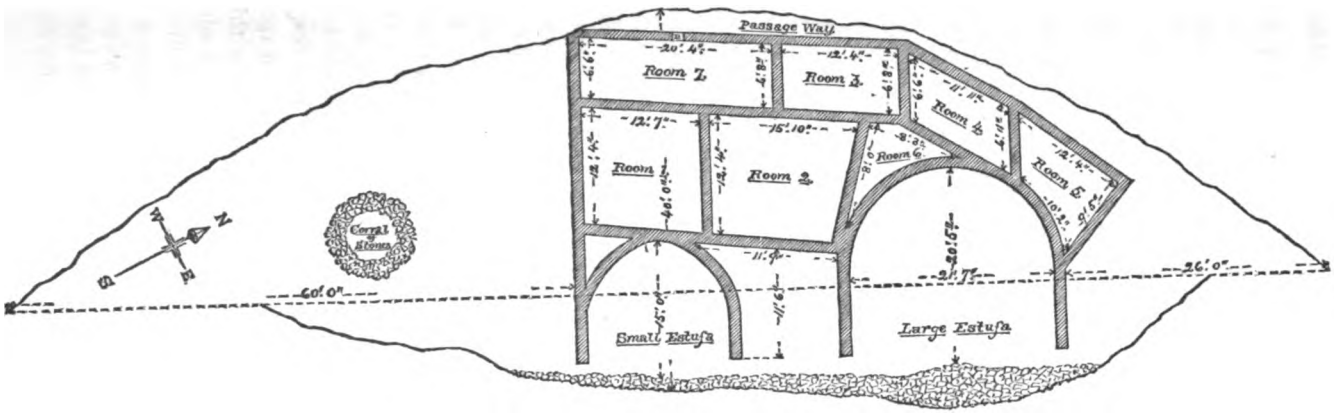
Fig. 1.




GROUND PLAN OF CLIFF-HOUSE.



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the excavation was forty feet, and the length one hundred and forty feet, which determines the size of the floor. The height upon the outside was about thirty feet, as near as we could estimate it. In only one place does the wall of the house reach up to the roof of the cavern. At this point the wall is from eighteen to twenty feet high. This wall of the house has also helped to keep the roof from falling.

As shown in the ground plan (Fig. 1.), the length of the natural ural excavation is one hundred and forty feet, the depth forty feet. This structure commences sixty feet from the southerly end of the natural excavation, and occupies the whole central portion and most of the north part to within twenty-six feet of the north end. It shows eight rooms and two estufas in the ground plan, and six of these rooms, numbered in the plan, 1, 2, 3, 4, 5, 6, are two stories high. The six lower rooms are partly filled up and apparently were not more than five feet deep. Room 7, situated at the western corner of the house, is a long and narrow room, 20 ft. 4 in. × 6 ft. 6 in. In the western side of this room there is an opening (D) into a passage which runs along the natural wall. This passage, at the point where the door is situated, is about four feet wide. It narrows to the south until it reaches its narrowest part about twenty-five feet from the doorway. This passage has been enlarged by the chipping of the rock from the roof wall, caused by atmospheric action. The passage has an outlet at the northern end, but at the southern it meets the back wall of the cliff, thus making it necessary for any one going through the passage to pass through room 7. This tends to show that the easiest external approach was from the south side, and the most difficult from the north side. This passage way along the roof wall is only four feet wide at the widest part at the base, yet the roof rises so rapidly that a person can walk erect the whole length of the hall, some sixty-four feet. The wall for the first twenty feet, however, is down, except about one foot of the lower end of it.

The walls of the rooms average one foot in thickness, and are constructed of thin pieces of tabular sandstone with adobe mortar, and there is more adobe mortar than stone in the wall. These walls are nearly vertical, and the masonry work is fairly done, only a small part of the walls having fallen.

The four walls of rooms 2, 4, 5 and 6, and the circular walls of the two estufas were standing, to the height, at least, of twelve feet above the apparent level of the floor.

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The room next east of No. 7 is No. 1, a large room 12 ft. 4 in. × 12 ft. 7 in. This room, as aforesaid, had another room below it, which was probably used as a granary, as it was not high enough for comfort as a dwelling room, and we found some corn cobs in it, of which the specimens exhibited are samples.

Next to room No. 1 is a small estufa, fifteen feet in diameter. This estufa is not enclosed on the outside, nor are there any remains to show that it ever was so, but a still better proof is that the end of the wall is smoothly finished off with adobe, and shows no breaks. Whether they enclosed the open side with a lumber frame and covered the top in order to darken the room like those I have since seen at Taos, darkened by being placed underground, there are no means of judging.

Proceeding to the second row of rooms, we first have room No. 3, 12 ft. 4 in. × 6 ft. 8 in. It is the wall between this room and No. 4, which rises as high as the roof, a distance of about eighteen feet. Next to No. 3 is No. 2, a large room 12 ft. 4 in. × 15 ft. 10 in. There is a doorway in the middle of the south side of this room two feet high, and one foot, four inches broad. The lintel and the sill are of stone. Each is a thin piece of sandstone from one to two inches thick and one foot broad. This is a remarkable fact and worthy of mention, as the use of stone lintels is very rare in Indian architecture. Below the former flooring of the room, and in the ground story, there is set in the wall a large stone projecting eight or ten inches on the inside, and a foot or two below the former level of the floor. There was probably another stone similar to the first, placed two or three feet below it and used with it as steps to descend into the lower room.

Next to room No. 2 is an open room between the two estufas, which, on account of its position, and not being protected externally, I do not think was utilized. The size of the room, however, was 11 ft. 6 in. × 11 ft. 9 in.

Next to room 3 is room No. 4; this room is 6 ft. 6 in. × 11 ft. 11 in.

Next to No. 4 is No. 6, a triangular room formed by the walls of rooms 2, 3, 4 and that of the large estufa which makes a curved side on the east of the room. The dimensions of the room are 8 ft. × 8 ft. 3 in. There is a break in the wall between rooms 2 and 6, which occurs in such a way that we could see the

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remains of the flooring of room 6 which is still in place, while all the other floors have been removed. This flooring was made as follows: first were laid three or four round timbers of cedar, from three to six inches in diameter, from wall to wall in the masonry. Across these, transversely, were laid poles of smaller size, either close together or a few inches apart. When laid apart there was a third course of fragmentary pieces of split cedar, or willows, and on this was placed the flooring of adobe mortar from three to four inches thick. As to the other rooms, where there was a ground story, the evidence is very plain by the presence in the walls of the stubs of the beams, the large ones crosswise and the small ones lengthwise of the rooms. In this room in the south wall there is a small niche, about five feet from the flooring of the room, which, as we afterward found at the Taos pueblo, was probably used as a shelf for household articles.

Next north of room 4 is room 5, 9 ft. 5 in. × 12 ft. 4 in. This room is quite irregular, as it is bounded on one side by the wall of the estufa.

Next to room 5 is the large estufa. This room is very interesting; its diameter lengthwise of the excavation is twenty-one feet, seven inches, and from the wall to a mass of stone and rubbish which rises two or three feet above the floor of the estufa, the distance is twenty feet, five inches. The walls of this estufa are very finely rounded and, like those of the small estufa, are finished off at the outer end with adobe. The walls of the estufa are eleven feet high. The wall is not continuous, but at the corner of room 6 it breaks, one side overlapping the other, leaving an open space between, through which it is possible to pass into room 6. This break occurs about five feet above the floor of the estufa, and is therefore not shown on the ground plan. It should be here stated that the estufas commence at the level of the floor of the ground story of the back rooms, and rise to the height of both stories. At the height of five feet, where the second story of the back rooms commences, this break occurs; below this the wall of the estufa being continuous. The upper wall of the estufa above five feet, after being laid in the usual way, was coated with adobe plaster of a slightly reddish tint, while the lower part remained a plain wall showing more stone than the other walls and better work. Near the lower end of the wall, about eight feet from the ground, there is the impression of a hand in red plaster. A little

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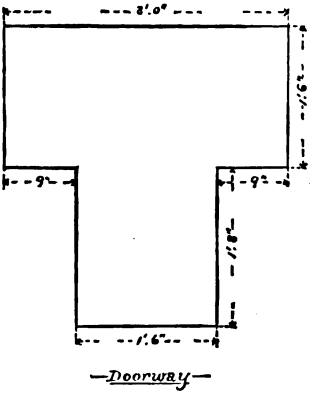
to the right of it there are three long, white, vertical stripes about six inches wide and two feet long. A short distance from these three there are three more like the first ones. At the point where the break occurs, but on the inside of room six, there are some drawings in white plaster, one representing a man with hands and legs extended. To the right of this drawing about seven feet from the ground there is a square window opening into room No. 4. To the right of this window there are three more stripes similar to the others. To the right of these stripes there is a doorway opening into room No. 5. This doorway (Fig. 2) is quite remarka-

Fig. 2.




ble; it is about eighteen inches wide at the bottom, continues of the same width to the height of about twenty inches and then widens, becoming from three to four feet wide. This change is abrupt, the doorway receding about nine inches on each side, thus making as it were two seats in the sides of the doorway. Above this the doorway extends about twenty inches. To the right of the door are two more impressions of hands, this time in white plaster. The front of this estufa is a large, solid rock and mass of rubbish, which rises four or five feet above the level of the floor of the estufa, enclosing it to a certain extent.

The position of this cliff-house is not to be imagined so inaccessible as the name seems to imply. Its access, after some investigation, although steep, still appeared comparatively easy. The face of the declivity is composed of a loose mass of debris, into which occasional trees and shrubs have taken root. Up this very



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steep slope, goats and even horses might ascend without much difficulty.

South of the main wall of the cliff-house there is a corral of stones (see Fig. 1), now fallen, with a mass of animal dung near it, about ten inches deep. It is impossible to determine whether it is the dung of horses, sheep or goats, but probably the latter, as the goat soon became a favorite animal with the Indians after the conquest. While the presence of these remains shows that the cliff-house in question was occupied after the period of the Spanish discovery, it does not establish the fact that the original builders continued to use the house down to so recent a period, as the remains may have been left by temporary occupants after the evacuation of the house by its builders.

This evidence of recent occupation, however, should have due consideration. At the commencement of the Mc Elmo cañon, twenty miles west of this point, there is a cliff-house with an enclosure for animals attached. Here also there is a deposit of animal dung about one foot deep, supposed by Mr. Mitchell, the resident ranchman, to be that of goats. This tends to the same conclusion of a modern occupation, as above explained.

Whether or not this cliff-house has been noticed in the government surveys of the territories, I do not know. I am not aware that it has been brought to public notice before the present time. John Gregor established his ranch in the Mancos valley, three years ago. He heard of the house in the spring of this year, and found it this year.

I have not been able, as yet, to consult the government report, but I understand that the cliff-houses, mentioned in the report as on the Mancos river, are lower down the cañon.

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## Description of two Stone Cists, discovered near Highland, in Madison Co., Illinois. By Arthur Oehler, of Highland, Ill.

[ABSTRACT.]

In the southeastern part of Madison Co., Illinois, in the southeast quarter of the southwest quarter of Section 10—Township 3, north, Range 5—west of the Third Principal Meridian, on the present property of Mr. Jno. F. Rodt, about four miles southeast of the town of Highland, two interesting cists, or graves, have been found. These cists are of the same shape and size, and lie in exactly the same direction. The first discovered, or No. I, lies about 400 feet south of the northern, and about 500 feet east of the western boundary of the above described parcel of land. Sugar creek, making a bend westward there toward the graves, is due east about 400 feet distant, while to the south it is considerably farther away. About two and a half years ago, the land having just been cleared (there is at present timber all around this parcel of land, except on the north side), the proprietor, while plowing, turned up several limestone rocks and fragments of slabs of different sizes, mostly small ones. This excited his curiosity, and last spring, with hoe and shovel, he made an investigation and discovered the grave. He then called the attention of my friend, Mr. Ad. F. Bandelier, to it, who, suffice it to say, as near as could be learned from the circumstances, reports: "That it contained the complete skeletons of two adult persons, one superposed upon the other." Mr. Bandelier then exacted from the proprietor the promise that if any more graves were found, he should immediately be notified, they, meanwhile, not being disturbed.

About May 20th, Mr. Bandelier received word that another, or No. II, had been discovered, and awaited his further pleasure. This second grave we carefully investigated.

It is situated about 350 feet due west of No. I, on the height of a hill that slopes southward toward Sugar creek, which is about a quarter of a mile due south. It has the shape of a rectangle, two feet three inches by three feet, the longer side lying north-northeast by south-southwest. It is made of limestone slabs; two at the bottom, two on each longer side, two on top as a lid, and one on each shorter side. These slabs show no signs of incisions or workmanship whatever. The covering stones are about a foot

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below the surface. There were five skulls and other bones of the skeletons of five adult persons in the grave. There was no trace of charring or fire about the remains, which had evidently been placed in the grave after the flesh had been removed. The skeletons show that the people were of rather small stature. The skulls lay in the position of a line drawn as a semicircle along the southeastern longer side. All the other bones lay transversely to the skulls, and were very closely packed. Some of them were so far decayed as to crumble while handling. There were no arms or ornaments found, except a small burnt, partially glazed object, having the shape of a cylinder, about two inches long and one-half inch in diameter, one little flint chip, and several fragments of limestone slabs. Three feet to the east of the grave is a stump of an oak tree, two feet in diameter, from which several roots, the thickness of a finger, penetrated the fissures of the slabs and worked their way between the bones.

One of the skulls was examined by Dr. Geo. J. Engelmann, of St. Louis. From this he judges that they were of great age, and that they appear as normal Indian skulls. A rather striking circumstance is that on all the jaw bones seen, the two front lower teeth are missing, as if artificially knocked out. On the skull examined by Dr. Engelmann, the left side of the parietal appears to show a rather prominent protuberance, while on the right is a corresponding flattening. Whether this is due to post-mortem causes cannot for the present be decided. As far as we know, this mode of burial is somewhat different from any yet described as having been practised by the North American tribes.

A great many flint chips, and arrow-heads of different shapes and sizes, are found in the neighborhood of these graves; also southward along the creek, which fact most probably indicates a former settlement.

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## Remarkable Burial Custom from a Mound in Florida—the Cranium utilized as a Cinerary Urn. By Henry Gillman, of Detroit, Michigan.

In opening a burial mound on the shore of Cade's pond, a small body of water situated about two miles northeastward of Santa Fe Lake, Florida, the writer discovered two instances of cremation in each of which the skull of the subject was used as the depository of his ashes. As this appears to be the first notice of the custom, if custom it may be considered, a brief description of the earth-mound and its contained relics may be deemed of importance in this connection.

The earth-work is about ten feet in height, and is situated fifty-seven feet northward from the shore of the pond. The north-and-south diameter measures eighty-eight feet, the east-and-west diameter, slightly in excess, has the length of about ninety-eight feet. The material of which it is built is the sandy soil, similar to that of the surrounding land. A tree of the yellow pine (*Pinus australis* Michx.) stands on the top of the mound, eight feet to the west of the centre, and measures, at two feet above ground, four feet, ten inches in circumference. The remaining timber growth consists chiefly of persimmon shrubs and small oak trees. The roots of the trees, particularly of the pine, had formed a net-work over the mound, and had penetrated to the bones and pottery, aiding in their destruction.

For a radius of twenty-five feet, and for a depth of from one foot to five feet, the mound was found to contain a large quantity of relics consisting of human bones, generally much decayed and representing a great number of burials, and finely wrought vessels of pottery, many of them painted in brilliant colors, chiefly red, brown and yellow, and some of them ornamented with indented patterns, displaying not a little skill in the ceramic art, though they were reduced to fragments. A stone axe and two or three chips of worked flint, found near the surface, were the only articles of stone which were encountered.

At twenty-one and one-half feet south of the centre of the mound, and six feet to the east, at the depth of two and one-half feet, a human skull was exhumed resting on its apex, the base consequently uppermost, or rather where had formerly been the base, for this part had disappeared. This skull was filled with fragments

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of blackened half-incinerated human bones mingled with dark-colored dust and the sand which invariably sifts into the skull under such circumstances. Immediately beneath the cranium lay the greater part of a human tibia having the peculiar compression known as platycnemism to an unusual degree. It is otherwise a well-developed bone; though the extremities having decayed prevented the obtaining of the perimetral index. The antero-posterior diameter of the shaft has a length of 1·560 inch, the transverse diameter a length of 0·800 inch, affording a latitudinal index of ·512. Beneath and surrounding this bone lay the fragments of a large number of other human bones, probably constituting an entire individual, but far gone in decay.

The second instance alluded to of this peculiar mode, in cremation, of utilizing the cranium of the subject as a cinerary urn, was discovered on nearly the opposite side of the mound, or at a point eight feet north of the centre and ten feet to the west of it. Here, at a depth of two feet, lay a human skull, as in the first example, base upward, and filled with a black mass—the *residuum* of burnt human bones, mingled with the all-prevading sand. In this case the work of decay was so much further advanced that the skull crumbled to pieces on being touched, and the contents were mostly resolved into a black ash or dust. At three feet to the eastward lay the shaft of a flattened tibia, the extremities wanting. This bone gives the following dimensions: antero-posterior diameter of shaft, 1·460 inch; transverse diameter, 0·770 inch; the latitudinal index being ·527.

It may here be stated that in both those cases of cremation the skulls were perfectly free from all action of fire, and were enveloped in the pure sandy soil of the mound, there being no surrounding ashes, nor appearance of outside fire; and though so decayed as to be very tender (in the second instance much more so than in the first), with the exception of the bones forming the base of the cranium, the different parts retained their shapes and relative positions, so that before the removal of the skulls, and their consequent falling to pieces, the writer had opportunity to observe their strong resemblance to the small orthocephalic crania which he had found to pertain to some of the mounds opened by him in Michigan. The same resemblance was perceptible in the other crania exhumed here. The small narrow frontal was particularly noticed, as were the prominent parietal protuberances,

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and the rather protuberant occipital, which was not in the least compressed. The supraciliary ridges were well defined, with the frontal rather retreating, and the superior border of the orbits presenting a quadrilateral outline; all the lower facial bones, including both maxillaries, being wanting. The walls of the skulls are of only medium thickness, varying from 0·1 inch to 0·3 inch.

The writer has in vain consulted such works as are accessible to him, with the object of deciding whether any similar relics have been exhumed from the mounds in Florida and elsewhere. He has found no record of the custom. Wyman mentions nothing of the kind among the results of his extensive explorations. There would seem to be no question, however, that these remarkable crania containing burnt human bones point to some ancient burial custom or superstitious rite.

That the skull in each instance belonged to the individual with whose incinerated bones it was filled, could not be positively determined, though the facts so strongly tend that way that there can be but little doubt of its being the case. Neither was it possible to decide whether all of the other members excepting the cranium were incinerated for the purpose indicated. In the first example, however, portions of the long bones, and particularly the greater part of the neck of a femur, were identified among the partially consumed fragments, while in neither case could the burnt fragment of a cranium be traced among the ashes.

Perhaps the facts here given may be thought suggestive of the custom, once not uncommon among some of the savage tribes of this continent, of sacrificing slaves or servants over the grave of a chief. This cruel practice, as is well known, extended even into the historic period. The close proximity of another burial in each of the cases of cremation here detailed might seem to support this conclusion. Or have we here the wretched evidences of the orgies attendant on some cannibal feast? It is interesting to remember that Wyman advances two facts, viz.: "cannibalism and the utilization of human bones," as being "consistent with the belief that Florida, for a time at least, was the dwelling-place of the Caribs." But, it must be admitted, there is no positive evidence here of cannibalism, though some of the human bones scattered through the mound appear to have been split open laterally, as if for the extraction of the marrow.

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## General Remarks upon the Archæology of Vermont. By George H. Perkins, of Burlington, Vt.

[ABSTRACT.]

The object of this paper was to present in brief review the various classes of, so-called, Indian relics found in Vermont, and drawings of many of the more peculiar forms were exhibited. These objects, though not occurring in Vermont in such profusion or variety as in many parts of the west, have nevertheless been found in many parts of the state in considerable number and variety. Rock inscriptions are rare, but two examples being known, one at Bellows Falls, where two large granite rocks near the Connecticut river bear, one a single large head, the other, and larger rock, twenty heads of various sizes, some with two ray-like objects extending upwards from the head, and one in the centre of the group with six. These figures are rudely outlined by a broad, shallow groove, the eyes and mouth being in most cases circular depressions, and in most the nose is left out. The largest of these heads is nearly twenty inches long, the smallest about five inches. The other specimen is at Brattleboro, and is known as "Indian Rock." This bears figures of eleven objects,—birds, mammals and snakes. It is near the junction of West and Connecticut rivers. Copper articles are rare, but chisels, gouges, spearpoints of different patterns, bars and short tubes, have been found, all of native copper, beaten into shape. Shell beads have been found in only one locality and are very similar to those found in the west. Pottery in fragments is not very uncommon in the western part of the state; but of entire jars I know of only two, though several others formerly existed, but have since been destroyed. In Vol. XXV, p. 325, of the Proceedings of this Association, the chief types of pottery are described and one of the jars figured. This earthen-ware seems to have always been made of pounded quartz, mica and other substances in varying proportions, and mixed with clay: and the finished jar received a coating of clay over both sides. It crumbled very easily, and on this account most of the jars, which when buried were entire, have fallen to pieces. It seems always to have been ornamented about the upper part, with impressed lines or narrow grooves, circles, squares, triangles, crescents, key-shaped figures, etc., arranged in a great variety of combinations, sometimes quite complicated, as

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may be seen by reference to the figure of the jar mentioned above. The color is most commonly dark red, or brown, sometimes becoming black. Pots of soapstone much more rarely occur and are usually unornamented. The usual varieties of stone objects are found in Vermont. Pipes are very rare. The few that have been found are finely finished and of handsome stone, but are not elaborately carved. Stone tubes, which by some are supposed to have been used as pipes, have been found, a dozen or more in all, varying from six to thirteen inches in length, and from one inch to nearly an inch and a half in diameter, the bore being small at one end and quite large at the other. Three of these tubes are figured on page 85 of the Portland Volume of the Proceedings. One end of these tubes is invariably stopped by a stone plug. Of "gorgets," "shuttles," or "two-hole-stones," many different forms occur. Nearly all of them are of handsome stone and are well made. Similarly shaped stones with one hole, and others with none, also occur. A single specimen of a quartzite discoidal stone, precisely like some that have been found in the mounds, was found in a grave at Swanton. "Banner stones" of various shape, some without, some with perforation, occur. Some of these are of very hard stone and all are finely finished. Boat-shaped stones of elegant finish are sometimes found, some very long and narrow, others shorter and deeper. Most of these are more or less hollowed out on the flat, or upper, side. Carved heads, somewhat bird-like, of smoothly carved stone, are rarely found. One of these is figured on page 95 of the Portland Volume. Of implements, the gouges are much the most carefully made and elegantly finished. A great variety of these articles has been found. Some are of basalt or other hard stone, but are nevertheless very regular in form. In some, the groove extends through only a part of the length, and in such cases the gouge is usually of about equal width throughout; in others, the groove extends from end to end, and some of these have an edge at each end. In some specimens both surfaces are flat, in others both are convex, sometimes slightly, sometimes very strongly. Most taper from the edge to the opposite end. They vary in length from four or five inches to over a foot, and no more elegant specimens than some of these gouges have ever been found in the state. In marked contrast with the gouges are the chisels, which are of ruder material and much less carefully made. Most are widest at the edge, which is usually well formed, the opposite

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end often being left with very little finish. Chisels made from hard stone are more cylindrical than those of soft stone. The length varies from a few inches to a foot or more. Some of the larger specimens are very clumsy. Axes are quite uncommon and are rarely grooved, or even notched, and are generally rudely made. Pestles of different forms are found, some tapering from the middle to each end, some much larger at one end than the other, some regularly cylindrical, others nearly square. A few have the upper end carved in the form of the head of an animal. Some are made of soft stone, but most are of trap, granite, or some hard material. Scrapers, rimers, knives, etc., are often found. Spearpoints of various sizes, styles and degrees of elegance are found, some very beautiful, others very rude and coarse. Of the smaller arrowpoints a great variety of forms occurs. All of the various types given by Foster are found. The most common form is triangular with, most often, a broad, indented base, but this graduates on the one hand into a very wide, low form, and on the other into a long, narrow form. The most common material is a bluish quartzite. Stemmed arrowpoints are abundantly found, barbed ones less commonly, and leaf-shaped points are rare. A very interesting and, I think, unique form has been found in several localities. In these the stem is notched on each side more or less deeply and regularly, while the surface is rubbed smooth and bevelled at the edges. The material of all these is slate, like common roofing slate. There is a very noticeable resemblance between many of the Vermont specimens and those from the mounds and other places in the west. Many of the two-hole stones, banner stones, scrapers, spear and arrowpoints, copper chisels and spearheads, are entirely identical in form with specimens from the mounds, but mounds like those of the west do not occur in Vermont, and we do not find here many forms which are not uncommon in the west. Spades and other agricultural implements are wholly absent, and I have found no evidence that the early inhabitants of Vermont were an agricultural people, or had fixed settlements of any great duration. It is singular that, while in every other class of objects we find much similarity to mound specimens, the pottery is notably different in form and ornamentation. Undoubtedly, many of our specimens should be referred to the Iroquois and Algonquins, portions of which tribes occupied the territory of Vermont when Champlain visited it in 1609, and con-

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tinued here until the latter part of the last century; but there is good evidence that many of the specimens were made and used by an older and different race—a wandering branch of the mound-builders as I believe, who had ceased their moundbuilding and agriculture and become a nomadic, hunting race, and modified their implements accordingly.

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## On the Sources for Aboriginal History of Spanish America.

By Ad. F. Bandelier, of Highland, Ill.

The history of the American aborigines has been a field actively explored for three centuries; and numberless data, or indications, have been gathered and used by all who claimed an active interest in the study. Thus they formed, to a great extent, the basis for numberless theories and hypotheses with which that field has been almost flooded up to the present time. It is not devoid of interest, therefore, however superficial it may prove, to attempt a critical review of the sources for aboriginal history, of a portion of the New World, which have thus accumulated.

In this review we cannot aspire to completeness, neither can we claim absolute impartiality in our judgments, or rather, "objectivity." All we endeavor is to follow the lead of such eminent scholars as Gregorio Garcia, Clavigero, Robertson, Prescott, Squier and Brasseur de Bourbourg; and if the very imperfection of our work should cause others to enter the same field, with the view of correcting our mistakes, we should feel happy and contented.

The actual drift of scientific inquiry requires that every study should have well-defined limits; and, the more limited the more *positive* (although perhaps not *striking*) will be the results of the investigation. We regret to say that in the actual state of American aboriginal history and of our knowledge of its sources, we can hardly circumscribe the field to a sufficient extent. Nevertheless, by selecting the "Sources for aboriginal history of Spanish America" in particular,—though we may attempt more

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than our limited resources really admit of—still it is a portion only of the general area, and possesses the advantage of presenting all the various kinds and degrees of records which can be used for historical purposes on American topics.

Among these, it is but natural that such contrivances as are, or were used exclusively by the aborigines for recording events (or rather as guide-posts to remembrance) should first occupy our attention. A glance at their value as historical sources is the more useful as it enables us to dispose, at the very beginning, of a number of records which have become a basis to more recent documents.

The incisions, or paintings, on the smooth surfaces of conspicuous rocks, are of frequent occurrence in Spanish America. We recall here the uncouth figures visible on the banks of the Orinoco, in Venezuela; some carvings still extant in the United States of Columbia; and especially the rude paintings near the now extinct volcanic basin of Masaya, in Central America. These are far from exhausting the number of such primitive monuments; but their manner of execution is typical of the kind. Furthermore, the last mentioned, namely: the Nicaraguan paintings of Nihapa, are very instructive. They exhibit conspicuously the *feathered serpent*. Coupled with the identity between the Niquiran language and calendar, and the Mexican idiom and chronological symbols, it is a valuable hint of the manner whereby such pictorial artifices might, in South America also, become even of ethnographical value. But it would be a grave error to attribute, to such imperfect attempts at commemoration, any greater importance than that of mere indication.

Of all the primitive devices invented, or borrowed, perhaps, by the American aborigines for the purpose of assisting remembrance, the Peruvian string-bundles, or "Qquippu," follow close upon the rude artifices just mentioned. The plan after which these singular contrivances are framed, though queer and original, is still simple and primitive. The "Qquippu" is but an expedient to commemorate *numbers* by the means of strings and knots. At a later period the *colors* of such were used to indicate also the *objects* whose numbers should be recorded. More than these two ideas object and number, the string-bunch of Peru cannot express. Event (or action) and time (or date), it is unable to denote. Consequently, it remains useless without the knowledge of definite

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oral tradition connected with it in each particular instance, and therefore the "Qquippu" is of little value as a historical source, unless the Qquippu-Camayoc" (literally, "keeper of their import") is present to state both the narrative and the chronological elements. Now, at the time of Garcilasso de la Vega, such "wise old men" were still living, and it was not improper, therefore, on his part, to mention the string-bunches as historical sources. But since his time, in the first two decades of the 17th century, the work of "Extirpation of Idolatry in Peru," was carried out, as recorded by Father Paul Joseph de Arriaga; and this not only destroyed the majority of "Qquippu" extant, but also frightened the natives into uncommon reticence about their meaning. Since then they have rapidly fallen into disuse, and the term "historical Qquippu" is no longer applicable. As a simple counting-machine they yet remain in use among the natives; but it is vain to look to string-bundles of ancient origin for any historical data whatever unless, as the case might be, oral traditions should have perpetuated themselves among the natives in whose hands they should be found.

We meet, on the Pacific coast of Peru, with symbolical paintings on pottery as well as on textile fabrics, but the interpretation of these pictures is yet purely conjectural, the more so since the language itself of the people by whom they were made and used has almost disappeared.

In order to find the highest development in the art of commemorating events among the aborigines of Spanish America, we must turn to the north of the Isthmus of Darien, to Central America and to Mexico. We cannot refrain from mentioning the fact here, without attempting any explanation, that the very highest attempts at recording events by American aborigines, are clustering upon an area forming the geographical division between the northern and the southern portions of the American continent.

As early as 1516, the Spanish officials at home had been informed, from Darien, of the existence of tribes which used a certain mode of paintings or drawings for the purpose of recording events. The word "book" was used then to designate such contrivances, even after a number thereof had been brought over to Spain and inspected there by learned men of the time. As early as 1520, the conviction was already expressed that the signs, exhibited by such paintings, were analogous to Egyptian

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hieroglyphics. Thus the existence of an aboriginal system of symbolical pictures in the New World became known, and the idea of "books" being already connected with it, opened the door to the conception of "picture-*writings*," which since has become a household word. When, furthermore, the discovery of figurative carvings on Central American ruins (noted by Palacios as early as 1576, at Copan) was connected with the Greek alphabet even by Garcia, 1606, the gradual ascent towards a fanciful interpretation began which through the Arcanæ of Father Kircher, the prudent hints of Humboldt and the bold vagaries of Felix Cabrera, finally culminated in the view that the symbols and figures of Central American pictorial art could be interpreted after the phonetic system of an alphabet. This theory of the celebrated Abbé Brasseur de Bourbourg was based upon the statements of the Bishop Diego de Landa concerning the Maya character of Yucatan.

Through the publication of Landa's *original* text, M. Brasseur has rendered an everlasting service to science. But in his own conception of that part of it relating to the so-called "letters" or alphabetical signs, he was rather unsuccessful. (We need no better proof of it than his subsequent explanation of the Codex Troano, which since has turned out to be a mere calendar.) Landa indeed asserts that certain signs were used by the Maya with a phonetic value attached; but on the other hand his own explanations prove that, in their unimpaired native state, the aborigines of Yucatan had no knowledge of an alphabet. The latter gives simple signs for *sounds;* it is besides, always introduced and preceded by syllabical writing. Now in a syllabical writing, the *consonant*, as the least flexible part of the utterance, forms the "pivot," so to say, around which vocal sounds oscillate; and this pivot is transformed integrally into the phonetic alphabet; signs for independent *vocal* sounds being added. We recognize it more particularly in the oldest records of Semitic nations, as well as in the various stages of Egyptian paintings and writings. Now, according to Landa, the consonant is broken in two, its very *sound* decomposed, a special sign being used, as in the case of the letter *l* for the vowel *e*, and another one, in the same instance, for the mute *l* proper. Had the natives ever conceived sound writing they never would have resorted to such clumsy expedients. Landa's Maya alphabet, therefore, comes under the head of those adaptations of aboriginal devices continued by Europeans for the

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purpose of facilitating intercourse, and in order to propagate more civilized ideas; — which adaptations are plainly acknowledged by Garcia and others. It would, therefore, be unadvisable to attempt an explanation of Central American paintings, or carvings, on the basis of phonetism or syllabism; and any work relying on such interpretations must necessarily become subject to doubt and to a cautious, careful criticism. Nevertheless, it cannot be denied that these designs are in the highest degree symbolical; but not as a *writing*, only as a *painting* must these commemorative artifices be regarded and interpreted.

The paintings (or carvings) of Central America and Yucatan are commonly regarded as being of a higher order than those of Mexico. To ascertain the value of this assumption we must examine each group separately. We begin with the Mexican; not only because there are a greater number of these extant, but especially because their system is well known, and because the signs composing them were ascertained at an early date.

Such of the Mexican paintings as are termed "historical" exhibit, in their typical form, the following characteristics: —

1. A central figure or group of figures; mostly human forms.
2. Near the heads of these figures, other designs connected with them.
3. A fringe of signs of various shapes, more or less completely encompassing the central field.
4. Dots, and signs like flags or feathers, connected with these dots. These sometimes accompany the *central* figures, sometimes the border *alone*, and again occasionally *both together*.

The chronological signs or figures of the Mexican calendar became known at an early date, and it was noticed that they agreed with the designs mentioned as forming a *fringe around* the painting; in other words, that this fringe represented the chronological element of the picture, thus giving the *date* of the event. It was also ascertained that, in order to record the *name* of any object or person whatever, the Mexicans painted the things after which these were named; and this led to the further discovery that the figures placed *behind* the central groups, expressed the names of their persons or things. Finally, the dots and other marks thereto related were proven to be *numeral signs*. After such discoveries, the true import of the central figures could easily be inferred: they are allegorical designs, indicating either the *object* or the *action*, as the case may require.

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A typical Mexican painting, therefore, expresses:—

1. The nature of an event.
2. The participants in it.
3. The time of its occurrence.
4. The numerical relations attached.

Further than this, however, it cannot be expected to impart any information. The narrative element, which furnishes the bulk of historical knowledge, is not represented. Mexican paintings, therefore, may be compared to commemorative stones "in situ," on which only the dates and a short record of the events were graven. The stones would even be superior to the paintings, inasmuch as inscriptions on the former are clear and positive, whereas designs on the latter are allegorical, and, consequently, yet require interpretation. The key to such interpretations is furnished by oral tradition alone.

We have been thus far speaking of the typical form only of the Mexican didactic painting. Such types are found occasionally; and on glancing over the magnificent reproductions of Mexican Codices which Lord Kingsborough has published, we sometimes find plates which may be admitted as displaying the very typical perfection. But the great majority of Mexican paintings is far from evincing all the characteristics of the ideal; still, none of them can be understood unless we look among them for one or the other of the four elements above stated as composing the typical plate or picture sheet. Thus the bulk of the Mendoza Codex, as reproduced by Lord Kingsborough, consists of tribute rolls, in which but three elements appear: date, tributaries (participants) and numbers. Other leaves of the same collection are mere representations of customs and manners of the people, wanting of course the chronological as well as the numeral element. The historical paintings proper often crowd upon one single leaf several occurrences, and therefore abbreviate, so to say, all the four classes of signs required to give perfection. The Codex Telleriano Remensis contains ample evidence of it. The so-called Borgia Codex has some, yet unexplained, paintings of the true typical form. Many of the original Mexican picture sheets, besides, are so overloaded with fanciful ornaments, that the actual symbolical components can barely be detected.

Mexican *sculpture* shows plainly the character which we have ascribed to Mexican *painting*. The bass-reliefs, along the rim of the so-called "stone of gladiatorial sacrifices," are a procession of

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figures representing both the nature of the events and the names of the actors (single men indicating tribes). The great stone of sacrifice, improperly termed "calendar stone," which is inserted in the wall of the Mexican cathedral, bears on its face the very date of its inauguration (13 Cane, or 1479). The other figures graven on it are purely ritual, as the purpose for which the stone was wrought actually implied. What little is left of the carvings on the walls of Xochicalco shows a near approach to the typical form; although it is, in perfection of design, more closely allied, and forms the transition to Central American delineatory art proper.

Very few paintings of genuine Central American origin, or from Yucatan, are known to exist. It is possible that four of them may yet be in Europe, to wit: the so-called Mexican MSS., No. 2, of the once Imperial Library, at Paris; the "Dresden Codex;" the "Codex Troano;" and, according to our learned friend, Señor José M. Melgar y Serrano, of Vera Cruz, a fourth one, which he calls the "Codex Miró."

Of the Paris Codex very little is known. The Codex of Dresden, figured first in part by Humboldt, and afterwards copied entire by Lord Kingsborough, while containing distinct allegorical figures scattered over its leaves, has its surface covered for the most part with little squares, which take the shapes of rude designs of objects. These squares are disposed in rows and lines and are directly connected with dots and bars.

The Codex Troano, on its seventy pages, exhibits spaces covered with allegorical figures; and others which take the form of a more or less broad fringe only. Lastly the Codex Miró consists of a central figure, with square symbols around and partly across it.

It is the preponderance of the little squares over the central allegorical designs which makes Central American and Yucatan paintings appear so different from those of Mexico. To these squares, called "Katun" in the Maya language of Yucatan (from "Ka," fire, or "Kat," what is fixed for memory, and "tun," stone), the interpretations of Landa have been applied, to test whether they were alphabetical signs or not. This attempt has signally failed; and since, M. de Rosny, of Paris, has decomposed the "Katun" into numerals and calendar tokens. This discovery establishes the identity of the "Katun" in the Maya painting,



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with the chronological and numeral signs of the Mexican picture leaf, and removes one of the main objections to the assumption that the two groups are practically identical, and only differ in details. The central figures, allegorical and symbolical, are common to both; and nothing remains but the so-called names of these figures to make the Central American system the exact counterpart in plan and value, of the Mexican picture sheet. This becomes still more apparent if we turn to the carvings and architectural ornaments exhibited on the ruins scattered over the areas of Guatemala, Chiapas and Yucatan.

Among these we select as typical, the carved monoliths of Copan, at the confines of Honduras and Guatemala; the sculptured tablets, called of the "Cross" and of the "Sun," at Palenqué, in Chiapas; and the vividly colored paintings on stucco, which adorn some of the walls of Chichen Itza, in Yucatan.

The pillars of Copan consist in the main of a central figure, the face and bust of a man, fancifully dressed, which is surrounded by a framework of Katuns. The latter, running even across the body, thus presenting a striking analogy with the Codex Miró of Sr. Melgar. The feet of the statues protrude at the bottom, in front, but the sides of the pillars are covered exclusively with Katuns, thus exhibiting a preponderance of the latter similar to that shown in the Dresden Codex. We have here the three elements: object, number and date, clearly represented, even in the case of erect statuary.

The famous tablets of the "Cross" and of the "Sun," at Palenqué, are *perfect* types. In them, the Katuns appear reduced to a more or less continuous fringe or border, as are elsewhere the Mexican calendar tokens. The allegorical and symbolical groups of the central field are boldly prominent, as in the most typical Mexican pictures; and furthermore, the leading human figures appear connected with smaller ones, which stand to them in a relation strikingly similar to that occupied by the "names" on Mexican paintings.

The ruined walls of Chichen exhibit various kinds of symbolical representations. One of these is remarkably typical: it is the painting copied by Mr. J. L. Stephens, from the doorway of the Akab Zib, as the construction is called. Others, contained in other edifices, show such a close resemblance to the more complicated historical paintings of the Mexicans (as shown, for instance,

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by the Codex Telleriano Remensis), that Mr. Stephens, one of the greatest authorities on Central American archæology, exclaims: "Among them are seen designs and figures which call forcibly to mind the well known picture writings of the Mexicans; and if these analogies are sustained, this building attached to the walls of the Tennis Court stands an unimpeachable witness that the people, who inhabited Mexico at the time of the Conquest, belonged to the same great race which furnished the builders of the ruined cities of Yucatan." The only objection of any value whatever, raised against these analogies, was based on the hypothesis that the Katuns were phonetic or syllabical signs. Since, however, it is proven that the latter are but calendar signs and numerals, the analogy mentioned by the great American explorer is not only sustained, but transformed into actual identity; and his remarkable suggestion becomes a fact recorded in words as well as in picture.

It is, therefore, utterly useless to search among Central American relics for any hidden art of pictography, or lost knowledge of writing beyond what is positively known of Mexican paintings proper. The only difference existing between the two groups is one of *details*, and not of *principle* or *system*. The degree of development was about equal in both; only, as well as the languages of the people differed radically, so did the chronological signs and the numerals, where the expression of higher numbers was required.

But after we have once determined the true nature of such didactic representations, and found them identical so far in Mexico, Chiapas, Yucatan and Guatemala, the question arises: How and to what extent can they be made available as historical sources?

The truly narrative portion is wanting in all these paintings. Oral traditions have to supply this want. Such traditions were numerously preserved, and many of them are recorded as explanations to the Codices above mentioned. The combination of such traditional records with the paintings, gives to the latter their true value. It is stated, for instance, that a certain event actually took place, but this event is perhaps related in various ways, and even so much disfigured that it is almost impossible to ascertain the truth. We quote, as an example, the occurrences immediately preceding the formation of the Valley Confederacy of Mexico, and the war between the Mexicans and Chalco. Both of

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these events, however, are represented in the Codex Telleriano Remensis, together with the dates when they occurred, and these paintings are indications as to *that* version which is really authentic. But any attempt at interpretation of like paintings, where such definite traditions about the events or occurrences are not extant, must be carried on with the greatest caution, always remembering how far the painting may be reliable, and carefully avoiding any tendency towards adding fanciful or foreign notions. The history of some of the better known Mexican picture leaves implies many warnings on that score. We recall here the erroneous interpretation of the so-called "Aztec Migration;" a painting likely from *after* the *Conquest*, which was introduced to the scientific world by Gemelli Carreri, two hundred years ago, as exhibiting the wanderings of the Mexican aborigines from Asia to their ultimate abode in America. The late Sr. José F. Ramirez, of Mexico, has conclusively proven that it but sketches the shifting of the Mexican tribe about the lake basin of Central Mexico. The mistake of Gama in explaining the sculptured rock called "Montezuma's watch," as a gnomon or calendar stone, whereas, as its very face tells us, it was but the stone of sacrifice, is still more instructive. Had the great Mexican scholar consulted native tradition about the stone, as embodied in the "Mexican Chronicles" of Tezozomoc, he would have seen how that "stone of the Sun" was made and for what purpose, how and when inaugurated and whereto it was subsequently cast away. It has fallen to the lot of another learned Mexican, Sr. Alfredo Chavero, to correct the errors of his distinguished countryman, and in this he succeeded only through a careful, critical study of Mexican aboriginal traditions which, besides Tezozomoc, Diego Durán, and the anonymous author of the Codex Ramirez have preserved.

From the remains of American indigenous commemorative art, we now turn to native recollections; to myths and traditions of the Indians of Spanish America.

Here we must premise that very few myths can be found which extend outside of the scope of *stock language*. Every creed, of course, embodies historical elements, and it would seem that among the mythological figures of every tribe it would be easy to pick out such as were almost identical with others of remote countries; thus indicating old historical connections. The analogy is indeed striking between the Tonapa or Tarapaca of Bolivia, the

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Nemtérequeteba of Bogota, and the Quetzalcohuatl, Cuculkan and Gukumatz, of Mexico, Yucatan and Guatemala. Nevertheless, we should be greatly mistaken if, as some distinguished investigators have attempted, we were to identify these different mythical personages with each other, and to base upon that assumption an early history of this continent. We must never forget that all such myths are to be studied in close connection with the places and with the sources where they were current among the people, and that, ere we permit ourselves to reach over into the realm of comparative generalization, a thorough knowledge of the subject must be secured. In most cases such an investigation will not achieve striking results at once. Thus, out of the five instances above quoted, three relate to the same: to Quetzalcohuatl, a historical figure of Mexico and Central America. Nemtérequeteba again evidently is a symbolical myth, an impersonation of the struggle for life in certain tribes; whereas the tradition of Tonapa may be regarded as even of doubtful authenticity, as far as *Indian origin* is concerned. With equal propriety we might use the tale of the fructiferous wand, found both among the southern Indians of the United States and in Peru, to prove that the Inca Chief, Manco Ccapac, once travelled out of the New Mexican pueblos to the Cherokees of Georgia. The tale of the *flood* is wide-spread, but as early as 1585, Father Joseph de Acosta wrote as follows: "that it cannot well be determined whether it relates to the general deluge treated of by the scriptures, or only to some local flood of which, as expert men relate, there yet are abundant traces visible." This utterance of one of the leading authorities of the 16th century on American aboriginal history should be taken into account, by such as have applied the local traditions of Guatemala and Yucatan to the myth of the Atlantis.

As a general rule, the more limited the area is over which a myth or tradition extends, the more likely is its authenticity and the greater its historical value. Such traditions form the actual basis of the histories written and published since the Columbian era, and it might be deemed natural now to turn to such writers of Indian origin as have preserved them in their works. Still, wherever the author of a work is known, he should, in this review, be treated in his proper chronological place, since he is a child, not only of his *race*, but also of his *times*. At the period when the Ixtlilxochitls wrote, when Tezozomoc composed his chronicles,

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and Salcamayhua gathered the tales of the Aymará and Qquichua tribes of ancient Peru,— the influence of European culture already weighed heavily on the natives. Consequently, these native authors cannot be torn from their natural connection with contemporaneous literature on America.

But there still exist some tales, or gatherings of tales, for which unimpaired aboriginal origin is claimed, and an origin at the same time so old that no definite authorship can be assigned to them; they are, therefore, regarded as typically pure productions of Indian remembrance, strictly authentic, and of corresponding authority for historical purposes. Four of these are particularly known. They are: for Peru, the poetical episode of *Ollantay;* for Yucatan, the series of *Katunes*, preserved to us by Don Pio Perez; and for Guatemala, the "*Rabinal Achi;*" but especially that bulky collection of traditions called "*Popol-Vuh.*"

"*Ollantay*" purports to be a romance, or dramatic episode, relating events which occurred before the Conquest among the Qquichua tribes on the higher Amazon, and at a time when the Incas were extending their sway in that direction. It is commonly represented as the tale of a bold rebellion against the power of the tribe of Cuzco, but it may equally well be that of a protracted resistance against said tribe. We must not forget that this tradition has passed through many European hands, and the suspicion is therefore not unfounded, that its present shape of a popular drama, calculated to give a high opinion of poetical art among the Peruvian natives, is due especially to the well known efforts of the clergy to promote dramatic art among the aborigines, in the same manner as children are trained to recitations and theatrical performances. Subsequently, "Ollantay" has been handled by several translators, who took the shape in which they found it for the one handed down from *before* the Conquest. All these successive handlings have covered the original tradition with a coat of foreign varnish, beneath which it is not easy to discern the real substance. As a *pure* specimen of aboriginal poetry it must, therefore, be rejected; as a historical tale it is limited to the locality where the tradition existed, and the more so since the Spanish historians proper of the Inca tribe ignore even the name of its principal actor. Were we able to divest "Ollantay" of everything which has accumulated around it during the past three centuries, it certainly would reveal important ethnological

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data; but we are convinced that these data would not confirm the notions now current about its real character and about the scope of its historical information.

Somewhat analogous to "Ollantay" is the "*Rabinal Achi*," a so-called popular play of one of the tribes of Guatemala. Here too the scope is limited, as well as the historical information. But the "Rabinal Achi" is valuable as a source for linguistics, and in this manner, on the territory which has witnessed its performance (however the latter may have become transformed through foreign influence), a territory broken up into a variety of dialects, with strange stock languages occasionally impinging, it still remains an historical source of some importance.

For the sake of analogy, we have inverted our former order of enumeration, placing the "Rabinal Achi" *before* the chronological history of Yucatan or "*Series of Katunes*," of the Maya Indians.

This singular document, written presumably about the middle of the 16th century, by a native of Yucatan, in the Maya language but with European letters, has been found and preserved by the enlightened magistrate of Peto, Don Pio Perez. Its publication outside of Yucatan, first in the original and in English about 1844, subsequently, in the original with a French translation in 1865, recalls two illustrious names in American archæology, — those of John L. Stephens and of Charles Etienne Brasseur de Bourbourg. Hardly any doubt can exist as to the authenticity of the piece. But it is so strictly confined to the enumeration of dates only, that very little can be gleaned from it beyond the fact that a certain chronology existed, for which, however, the "Codex Pio Perez" itself gives no point of departure whatever beyond that during the *ninth* "Ajau," the Bishop Toral arrived in Yucatan. Fray Francisco de Toral lived in Yucatan between the years 1554 and 1571, and this is a slender basis for a connected chronology. Still it is a point of departure, and in this respect the "Series of Katunes" might furnish approximate ideas. But another difficulty arises out of the *geographical* names. A careful examination of these must convince us that some of them belong to a language *different from that in which the document is written.* Although many localities can easily be identified in Yucatan, many others are still in doubt, and the "Series of Katunes" requires further scrutiny ere it can become a reliable source for historical purposes.

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But *that* gathering of Indian myths which has, within fifteen years, obtained the greatest celebrity, is the "*Popol-Vuh*," called by its illustrious translator, Mr. Brasseur de Bourbourg, "The Sacred Book." We cannot help differing at the very outset, from the learned "Américaniste," in regard to this title. Indian names and terms should be taken in the sense which Indians put to them, and not with hints at Egyptian terminology for instance. Thus the name of "Popol Vuh," according to M. Brasseur's own vocabulary, does not signify a holy writing, but simply "a gathering of leaves."

The "Popol Vuh" does not bear, on its face, any date whatever, but at the close of it there is a statement which fixes the time of writing of the tales therein contained, at about the second half of the sixteenth century. Still it was only during the seventeenth century that the dominican friar, Francisco Ximenez, discovered, at the hamlet of Santo Tomas Chichicastenango in Guatemala, an obscure manuscript, written in the Qquiche dialect of that country. This document appears to have been the original of the present "Popol Vuh." The original itself has since disappeared; but Fray Ximenez wrote, about 1720, a "History of the province of the Predicant Friars of San Vincente de Chiapas and Guathemala;" also a "History of the origin of the Indians of this Province of Guatemala, translated out of the Qquiché into the Spanish tongue." (Both of these works seem to be extant at Guatemala, although they were never printed.) Into the former of these bulky manuscripts (originally composed of four volumes in folio), the Qquiché text of the "Popol Vuh" has been copied by Fray Francisco Ximenez. He also made a Spanish translation, but, although he was so familiar with the Indian idioms of that country as to use them like his mother tongue, his work in that respect is far from satisfactory to M. Brasseur de Bourbourg. Truly, the language is almost too fluent and too fine to be Indian.

Father Ximenez saw, in the introductory parts of the "Popol Vuh," a dangerous plagiarism, committed by the devil on the Mosaic Genesis. There is nothing which proves that *his* copy should not be exact. But it is well to register the fact that he himself is struck by the concordance, or rather analogy, existing between the first parts of the "Popol Vuh" and the most ancient records of Semitic tribes.

The value of whatever is clearly aboriginal among the traditions

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thus preserved by Ximenez was felt, rather than ascertained, by subsequent ecclesiastics of Central America and Southern Mexico; and towards the close of the last century, or at the beginning of the present one, Don Ramon Ordoñez y Aguiar wrote a "History of the creation of Heaven and Earth," the manuscript of which is still preserved in fragments at Mexico. It is an accommodation of the contents of the "Popol Vuh" to dogmatic notions of the Church. In 1794, Don Felix Cabrera made further use of them for his "Teatro Critico Americano," in which he attempts to prove the descent of the Central American aborigines from the Semitic nations. In 1857, Dr. Charles Scherzer at last published the Spanish translation of Ximenez; and finally, in 1861, M. Brasseur de Bourbourg gave to the scientific world the Qquiché original, with a French translation opposite; and since that time the "*Popol Vuh*" has remained, according to the side which is taken, either a suspicious riddle, or a valuable document for the most ancient history of Spanish America.

There is nothing in the document itself to give any clew to its author. The time when it was written can be approximated as aforesaid, since there are two Indian chiefs of Guatemala mentioned with *Spanish* names, *one* of which is stated as being in office at the time when the document was written. It fixes that time about the second half of the sixteenth century. That it was executed in Indian style there is no doubt. In that respect it comes under the same head as the "Codex Chimalpopoca," composed in Mexican in 1558,—as the so-called works of the Ixtlilxochitls of Tezcuco (previous to the last writer of that name) and others;—all written in native languages with European letter, towards the middle of the sixteenth century. That the writer of the "Popol Vuh" may therefore have been an Indian, there is no serious reason to doubt. But there are other considerations looming up here.

We have already mentioned that the original of the "Popol Vuh" is no longer existing. Moreover, a full century elapsed between the writing of that original and the time of its recovery by Friar Ximenez. The Spanish translation of the latter shows important discrepancies with the Qquiché text, and the question therefore arises: has that text remained intact up to the time when Ximenez incorporated it in his manuscript? This question we cannot answer, but the next one suggested is: how far are the

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traditions related, of true Indian origin? An examination of the contents of the document may throw some light on the subject. The "Popol Vuh" consists of four parts. The first and second relate to cosmogonical myths, to which are finally attached a number of Indian tales proper, among which certain notions of Indian institutions reveal themselves. The third and fourth parts record, Indian fashion, the story and fate of the Qquiché tribe of Guatemala, from a remote period down to the Spanish Conquest.

The first part is unique. Grand cosmogonic conceptions, clad in noble and impressive words, taper off into the unshapely fairy and monster tales of Vukub Caquix and of his sons. There is a surprising discrepancy between the opening and the close of this first section of the "Popol Vuh,"—so startling even that it cannot help arousing our suspicions. They appear to have been written by the same pen, but to have been *differently inspired*. Besides, the analogy of the purported Qquiché cosmogony and the Genesis is so striking that we perfectly well conceive how Ximenez could explain it as a work of the devil, in derision of the christian faith. We cannot resist the temptation to give a specimen of this remarkable production.

"This is the first book (or leaf) written in olden times, but its sight is concealed to him who sees and who thinks. Its appearance is admirable, as the tale of the times in which everything in heaven and upon earth completed its shape, the signs, the measure of their angles and their distribution, also the parallels in the skies and on earth, at the four ends and four (cardinal) points,—as it was spoken by the Creator and the Moulder, the Mother, the Father of life and being, by whom every one lives and breathes, Him the Father and living source of the peace of the nations of His well-bred people. He, whose wisdom has planned the excellency of everything that exists in the sky and on earth, in the lakes and in the sea.

This now is the tale how everything was in suspense, everything was calm and silent, immovable and in peace,—the immensities of heaven being void.

Now this is the first word and speech: There was neither man, nor animal, neither bird nor fish or crab, no wood, no stones, no ravines, no thickets or herbs or shrubbery,—the heavens alone existed.

The face of the earth did not manifest itself yet, the peaceful sea alone existed and the spaces of heaven.

There was yet nothing which formed a body, nothing which held together, nothing that moved, not the least sound was heard in the skies.

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Nothing stood erect; the peaceful waters of the sea, calm within its limits, alone were, because nothing existed.

Nothing but the immobility and silence in darkness and night. But alone the Creator, the Moulder, the Ruler, the feathered serpent, those who engender, those who give being, they are on the waters like a growing light.

They are wrapt in green and in blue, therefore Gukumatz is their name; his being is of the deepest knowledge. This is how the sky exists, thus also is the heart of heaven; such is the name of God, and thus he is called.

Then it happened that the word (Brasseur translates this "his word," but Ximenez has it "the word") came thither, with the Ruler and the Gukumatz, in the darkness and night; and it spoke to the Ruler and to Gukumatz.

And they spoke: then, they advised together and pondered over it, they understood each other, joining their words and opinions.

And while they were thus advising together, dawn appeared, and as day broke, man made its appearance, whilst they were consulting upon the bringing forth and growth of the woods and vines, upon the being of life and mankind in darkness and night, —through him who is the heart of heaven, whose name is Hurakan.

Its first token is lightning, its second one thunder, its third is the deadly ray; and these three together are of the heart of heaven."

We can hardly fail to notice, besides the great similarity which some of the passages quoted exhibit with the well known words of the Mosaic traditions, the striking use made of the term "Word" in the sense of the "Logos" in the "Gospel of St. John." Whatever signification may be attached to the "Logos," its conception as an acting individuality, a personal spiritual being, is certainly not Indian. That the "Popol Vuh" is, therefore, in its first sections, a suspicious document (to say the least), is very probable. It is very singular that even the most diligent gatherers of Central American traditions, like Las Casas and Garcia, make no mention of the cosmogonic tales apparently preserved by the "Popol Vuh." The suggestion of a so-called pious fraud, or rather of a modification of Indian myths for the purpose of alluring the natives into conversion, is therefore plausible. This impairs at once the value of that part of the "Popol Vuh" as an historical source. In the remainder of that first section, M. Brasseur has thought to see geographical myths, or rather the record of great natural convulsions, whose forces appear impersonated in the figures of Vukub Caquix, of Zipacná and Cabrakan. His opinion is sustained by

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the fact that some of the names given by aboriginal dialect to volcanic peaks of Guatemala correspond to those of the heroes of the "Popol Vuh." But this in turn proves that the convulsions of nature supposed to be related were *local* phenomena only, the myths themselves but *local* myths,—and thus the theory (or hypothesis rather) falls to the ground, which identified the Atlantis with the West Indies, and which attributed to that ill-fated mythical Island the "peopling of America." That the second half of the "Popol Vuh's" first part records genuine Indian myths cannot be doubted: two celebrated authors, nearly contemporaneous with the probable date of writing, the "Popol Vuh," the Franciscans Mendieta and Torquemada, both attest their authenticity.

As soon as we turn to the remaining three sections of the "Popol Vuh," we are on very valuable ground for historical purposes. They contain the plain history of an Indian tribe, recorded by one of its members who fully understood its organization and institutions, and who consequently gives a true picture of the development of such institutions up to the time of their overthrow by the Spaniards. We do not hesitate to say that American aboriginal history has no document or source whatever, which is more valuable than the three last parts of the "Popol Vuh;" but we are equally convinced that a closer study of the Indian language, in which it is composed, is absolutely needed. A new translation of it should be undertaken. Such a translation, if based upon a thorough knowledge, not only of the philological elements of an idiom, but also of the train of thought and state of society and family among the nation who spoke it, must draw from the "Popol Vuh" facts, which will settle some important questions of aboriginal history in Spanish America.

The "Popol Vuh," however, exhibits one peculiarity. It is devoid of any and all *dates*. A vague genealogy may, coupled with striking events otherwise ascertained, ultimately cover this defect to a certain extent; but that very infirmity has still a redeeming feature, because, in its present shape, the text of the "Popol Vuh" cannot give any show to inconsiderate enthusiasts towards the fabrication of an uncouth imaginary chronology. The "Popol Vuh" has already been so largely misused, whereas it is *so little known*, that any quotation from it must remain suspicious, so long as the author does not give any satisfactory evidence of his ample knowledge about the document and of its history proper.

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Reserving, as we have already said, the bulk of authors of American aboriginal origin for their chronological position in the series of sources produced after the conquest, it remains for us to cast a passing glance at several traditions, myths and actual "legends," current among nations and races, which, at the time when Columbus discovered the shores of America, were settled *outside* of that continent. They are all of very old date, and the oldest one, at least as far as its recording is concerned, is the Greek myth of the "*Atlantis*." I call it a *Greek* tale since Greek authors have preserved it, although, from its purported origin, it should properly be defined as *Egyptian*. We would not have referred to the Atlantis here, had it not become the subject of much speculation, within the past twenty years. To such as have indulged in the fancies which the celebrated dialogue in Plato's Timaeos may appear to foster, there is little consolation to offer. The results of the Challenger expedition have scarcely left any show for that great wreck, which the destruction of the Atlantis should have scattered between Europe and America; and Sr. Naranjo y Gil appears to have conclusively settled the question about a possible connection in origin of the Canary Islands with a sunken area of greater expanse. The Atlantis myth was thoroughly sifted in the sixteenth century, and the results of such long discussions have been always negative, though with a timid doubt attached to them; or an "interrogation point" rather. It is singular indeed how long this tale has maintained itself before the scientific world. Forgotten during centuries, it turns up with increased vitality at the latest period, to be utterly refuted again. Nevertheless, it leaves an indelible mark in the aboriginal history of Spanish America. Whether this mark is but the scar of an ugly ailment, or whether it is a small germ, often crushed, though never extirpated, of future discoveries, we are not called upon to investigate. My own predilections and convictions incline strongly *against* the Atlantis. Still, I cannot refrain from mentioning the fact that even among the aborigines of Australia, the tradition of a continent now submerged, but having once existed to the *East* of theirs, has been found.

The history of the propagation of Christian dogmas in the British Isles has left to us a legendary tale so far incorporated in the annals of the Church of Rome, as to become connected with a particular day of its ritual calendar; the sixteenth of May. It is

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the legend of St. Brandan's voyage to a western paradise. The facts reported by this legend and those connected with the adventures of his disciple St. Machutes or St. Maclovius, during a trip to the Western Island of Ima, appear so fabulous, that they do not deserve particular mention. Still, while they are found repeatedly in various manuscripts written between the tenth and the fifteenth centuries of our era, they finally localized in the Islands of Madeira and of the Azores, where, at the time of Columbus, they were found in the shape of the fairy Island of San Boronden, reported to appear in sight towards the *setting sun*, at certain periods of every year. This connects it with meridional or, in general terms, with Spanish America. Such anticipations of a western world occasionally loom up in charts drawn long previous to Columbus in the shape of a large Island called *Antillia*, situated between Asia and Europe. We are unable, as yet, to define upon what basis of fact the latter indications repose.

Of the numerous reports contained in the "Sagas" of the Northmen about their voyages to western shores, but one — the tale of Gudleif Gudlaugson's adventures in an unknown land, far to the "west and southwest" from midway between Iceland and Ireland, whither he had been driven by north east winds — might be construed so as to allude to parts of Spanish America. There he met one of his countrymen, Bjorn Asbrandson, who had disappeared from Iceland in 999. Notwithstanding the disdain with which the Hon. Geo. Bancroft has persistently regarded the Northern Sagas about America, the perfect authenticity of the source, the "Eyrbijggia Saga," whence this story is taken, cannot be denied. The tale itself recalls forcibly the adventures of Alonzo Guerrero and Géronimo de Aguilar among the natives of Yucatan, between 1511 and 1519, which are perfectly authentic. Still, while in general the occasional visits of Northmen to some parts of America are established beyond a doubt, it would be highly imprudent to extend the scope of their cruises beyond the limits of its northern section. In the story of the voyages of Nicoló and Antonio Zeno, of Venice, and of their stay in a foreign western land between 1388 and 1394, we can easily discriminate between truth and fiction. While it is highly probable that at least one of the two brothers aforesaid visited *Greenland* about the close of the fourteenth century, the details which he is said to have collected afterwards about countries farther south, and which details would

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agree with the manners and customs prevailing in aboriginal Mexico, are evident interpolations made even subsequent to the time of Cortés.

Much stress has been placed, especially in some parts of Europe, on these so-called "pre-Columbian discoveries of America." While some of them can certainly not be denied, their importance for aboriginal history of Spanish America should not be overrated. If we compare the reports of such early voyages, and even the myths and legends just mentioned, with native American traditions, and especially with the degree of development and the customs of the aborigines, we meet with but very faint traces of analogy. Garcia, in his critical examination of the Atlantis myth, mentions the reported communal living of the "Atlantidæ" in connection with analogous customs among the Indians of Peru. In conjunction with the tale of the Island "Antilla," mention is made of the flight to its shores, in the sixth century, of the Bishop of Oparto with some followers. They are said to have founded there seven cities. This recalls the seven caves or grottos, from which the seven tribes or kinships of Mexico are said to have issued; the seven ravines of Tulan Zuiva of Guatemala; the seven openings in the rock at Paucar Tampu in Peru; and, finally, the seven New Mexican pueblos of Cibola. It is, as yet, unsafe to treat such indications otherwise than as interesting coincidences; and the influence exercised on the destinies of the American aborigines, by such possible landings of Europeans, has certainly not left a *permanent* mark. Like the Atlantis, and the protracted stay of the Northmen in New England about the eleventh century, they have even been forgotten by the natives, for it would certainly be very injudicious to identify the "Votan" of Chiapas with the "Wodan" or "Odin" of Scandinavia and Germany, or to acknowledge in the landing of Toltecs at Tampico an *Irish* colony on Mexican soil.

Only one European nation, the Swiss, has been, during the past four centuries, spared the honor of being treated as the progenitors of the "noble red man," or his forerunners on the soil of America. Africa must feel somewhat neglected on that score, whereas Asia has, out of whatever nomadic or semi-nomadic hordes ranged over its northern half, been called upon to contribute largely towards our aboriginal population.

While such theories and speculations necessarily remain foreign

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to the subject of this paper, the ancient literature of the Chinese Empire, however, possesses documents of unquestioned authenticity, which, through their interpretation by European scientists as relating to America, deserve to be mentioned here. We allude to the reports on the land of Fu Sang, situated far to the east of China, which region Mr. De Guignes, who in 1761, first gave a translation of the Chinese stories, identified with America. His work has been followed by successive affirmations as well as by disprovals, which, after 127 years still leave the Fu Sang problem (as we may call it in relation to America) in the same original condition of doubt and mystery.

But it is well to note here that the story itself, as a genuine production of a Chinese writer who lived about the seventh century of our era, has never as yet been subject to doubt. Its authenticity is so far unimpeached. The question, as to whether Fu Sang really means America or not, is the only one at issue. This question becomes very important from the fact that the Chinese annals claim to have gathered their knowledge of the country from Buddhistic pilgrims who resided there, the first of that class of priests having visited Fu Sang in 455, "when they propagated the religion of Buddha, scattering also holy books and holy images. They taught the people the rules of monastic living, thus changing their manners and customs." The time when such a Buddhistic mission to America is said to have taken place corresponds about with the dates assigned, in some histories of Mexico, to the settlement of Toltecs and the advent of Quetzalcohuatl. Unfortunately, there is very little in the numerous tales about the latter person, or about the religious ideas of Mexican tribes at his time, which reveal Buddhistic principles. In fact, there is no trace to be found of the latter in any aboriginal creed or mythology of the New World. Therefore, while it is very possible, and even likely, that Buddhistic priests may have visited America at an early date, their claim of having reformed its inhabitants, introducing a new religion, and thus wielding a powerful influence on their subsequent destinies, falls to the ground, and with it the Fu Sang tale becomes deprived of its chief importance. Early communications between the seafaring people of Eastern Asia and the western coasts of America probably occurred, and that Li Yen's book of Fu Sang really treats of such a communication appears to us very likely, notwithstanding the uncouth and fabulous details contained in it.

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Such exaggerations and misstatements but confirm its authenticity of origin and date. But the fact that such a communication is indicated does not necessarily make the document itself a valuable historical source. Still it is an indication of the possible existence of other similar sources in the extensive literature of China and Japan.

Having thus, not exhausted, but attempted to characterize, both the strictly aboriginal records and the scanty traces dating, among the inhabitants of other continents, previous to the close of the fifteenth century, we approach the time when the so-called New World was at last called upon to enter into continuous intercourse with the remaining parts of the earth's surface. The nations of Europe had long possessed the art of writing; the printing press had just been introduced among them, thus facilitating reproduction, and consequently diffusion of knowledge. When, therefore, at the end of the bold sailing of Columbus, he struck the shores of the unknown land, the fog lifted before the eyes of Europe only to uncover an uncomprehended riddle in nature and in history. With the resources then at hand, the intellect of Europe began to work at the solution of the problem which the New World offered, and is working at it still. The result of it has been a vast literature on American aboriginal topics, which literature will form the subject of a future communication.

NOTE.—It was my intention to give such annotations to this paper as would substantiate the statements made, but while the material for the notes was ready, they could not be fully written in time for the present publication. Thus, what I consider the essential part of the paper, is unintentionally omitted. It is my purpose, however, to reprint the series of papers, upon which I am now engaged, in book form at some future day, when the present deficiency will be amply remedied.—A. F. B.



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## TITLES OF OTHER PAPERS READ IN SECTION B.

NOTICE OF A RECENT DISCOVERY OF GOLD IN THE UNAKA MOUNTAINS OF TENNESSEE. By JAMES M. SAFFORD, of Nashville, Tennessee.

NOTES OF CERTAIN SULPHIDE SPRINGS IN TENNESSEE. By JAMES M. SAFFORD, of Nashville, Tenn.

THE GOLD REGION OF GEORGIA. By GEO. LITTLE, of Atlanta, Ga.

EXHIBITION OF FOSSILS, SHOWING THE EFFECTS OF THE GEODIZING PROCESS. By A. H. WORTHEN, of Springfield, Ill.

A NEW FOSSIL IMPRESSION IN A GLACIAL BOULDER. By A. R. GROTE, of Buffalo, N. Y.

ON HYBRIDS IN NATURE. By THOS. MEEHAN, of Germantown, Pa.

ON THE COMPASS PLANTS. By GEORGE ENGELMANN, of St. Louis, Missouri.

REMARKS ON THE GEOGRAPHICAL DISTRIBUTION OF THE LAND AND FRESH-WATER MOLLUSKS OF THE UNITED STATES, AND THEIR LOCAL VARIETIES. By A. G. WETHERBY, of Cincinnati, O.

ARE THE SO-CALLED CHÆTETES OF THE CINCINNATI GROUP BRYOZOANS? By A. G. WETHERBY, of Cincinnati, Ohio.

ON A REMNANT OF THE SPIRACLES IN AMIA AND LEPIDOSTEUS. By B. G. WILDER, of Ithaca, N. Y.

ON CERTAIN DIFFICULTIES MET WITH IN USING THE CAT'S BRAIN AS A TYPE OF THE BRAINS OF MAMMALS. By B. G. WILDER, of Ithaca, N. Y.

REMARKS ON THE RUINS OF A STONE PUEBLO ON THE ANIMAS RIVER, NEW MEXICO, WITH A GROUND PLAN.[1] By L. H. MORGAN, of Rochester, N. Y.

[1] This paper will be printed in the "12th Report Peabody Museum Arch. Eth.," Cambridge, 1879.



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OBSERVATIONS ON THE SAN JUAN RIVER DISTRICT, AS AN IMPORTANT ANCIENT SEAT OF VILLAGE INDIAN LIFE. By L. H. MORGAN, of Rochester, N. Y.

ANCIENT MOUNDS IN THE VICINITY OF NAPLES, SCOTT COUNTY, ILL. By J. G. HENDERSON, of Winchester, Ill.

ANCIENT NAMES, GEOGRAPHICAL, TRIBAL AND PERSONAL, IN THE MISSISSIPPI VALLEY. By J. G. HENDERSON, of Winchester, Ill.

ANCIENT POTTERY FROM CHIRIQUI, CENTRAL AMERICA. By O. C. MARSH, of New Haven, Conn.

DESCRIPTION OF A GLAZED EARTHEN VESSEL, TAKEN FROM A TUMULUS IN FLORIDA.[2] By HENRY GILLMAN, of Detroit, Mich.

EVIDENCES OF CANNIBALISM IN A NATION BEFORE THE AINOS IN JAPAN.[3] By E. S. MORSE, of Tokio, Japan.

AN ATLAS OF NORTH AMERICAN ANTIQUITIES. By O. T. MASON, of Washington, D. C.

NORTH AMERICAN INDIAN SYNONYMY. By O. T. MASON, of Washington, D. C.

ON AN ANATOMICAL PECULIARITY BY WHICH CRANIA OF THE MOUNDBUILDERS MAY BE DISTINGUISHED FROM THOSE OF THE MODERN INDIANS.[4] By W. J. MCGEE, of Farley, Iowa.

EXHIBITION OF PREHISTORIC RELICS FROM MISSOURI. By A. J. CONANT, of St. Louis, Mo.

ACCOUNT OF AN EXPLORATION OF A WALLED TOWN OF THE MOUNDBUILDERS OF THE CUMBERLAND VALLEY.[5] By F. W. PUTNAM, of Cambridge, Mass.

GEOLOGICAL HISTORY OF THE COLORADO RIVER AND PLATEAU. By C. E. DUTTON, of Washington, D. C.

[2] Printed in the "American Naturalist," December, 1878.
[3] Printed in the "Tokio Times," January 19, 1879.
[4] Printed in the "American Journal of Science," December, 1878.
[5] Printed in the "11th Report of the Peabody Museum Arch. Eth.," Cambridge, 1878.

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THE LANDS OF UTAH. By J. W. POWELL, of Washington, D. C.

THE RAINFALL OF THE ARID REGION OF THE UNITED STATES. By J. W. POWELL, of Washington, D. C.

DINOSAURIAN REPTILES FROM THE JURASSIC OF THE ROCKY MOUNTAINS. By O. C. MARSH, of New Haven, Conn.

DISCOVERY OF *Atlantosaurus* AND OTHER DINOSAURS IN THE ROCKY MOUNTAINS OF COLORADO. By A. LAKES, of Golden City, Colorado.

THE ALLEGED VOLCANO AT BALD MOUNTAIN, NORTH CAROLINA. By F. W. CLARKE, of Cincinnati, Ohio.

REMARKS ON THE EARLY DECAY OF THE MALE PLANT IN *Cannabis sativa*: A PARALLEL AND A CONTRAST. By C. S. PERCIVAL, of Marshalltown, Iowa.

EXTRACTS FROM MODERN SCIENCE BEARING ON THE LAW OF REPETITION. By MISS V. K. BOWERS, of Grand Eddy, Mo.

ON THE MEANS BY WHICH SILK-WORM MOTHS ISSUE FROM THEIR COCOONS. By CHAS. V. RILEY, of Washington, D. C.

ON *Ægia tipuliformis*. By GEO. H. PERKINS, of Burlington, Vt.

ON THE CONSENSUS IN ANIMAL AND VEGETABLE LIFE. By LESTER F. WARD, of Washington, D. C.

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The following title was omitted by mistake from Nashville Volume. The paper has been printed in Can. Ent., Vol. IX, pp. 161-165, 1877.

A NEW LEPIDOPTEROUS INSECT INJURIOUS TO VEGETATION. By AUG. R. GROTE, of Buffalo, N. Y.

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# EXECUTIVE PROCEEDINGS

OF

# THE ST. LOUIS MEETING.

## HISTORY OF THE MEETING AND GENERAL REPORT OF THE PERMANENT SECRETARY.

FOR the second time, during the thirty years of the existence of the Association, a meeting has been held on the western bank of the Mississippi. The 21st meeting was held at Dubuque, and just six years after the adjournment of that meeting the 27th was organized on the 21st of August, 1878, at St. Louis, Missouri. In this connection it is of interest to note that of the twenty-seven meetings one has been held in Montreal, Canada, one in Charleston, S. C., nine in the New England States, eight in the Middle Atlantic States and District of Columbia, and eight in the Central States.

Owing to the unprecedented heat from which the city had suffered, and to the epidemic of the yellow fever which was increasing to an alarming extent in the southern states at the time of the meeting, the attendance of members from the north and east was much lessened by the fear that the epidemic would extend up the river to St. Louis; and this fear was increased by several erroneous and sensational reports that the fever had broken out in the city during the week preceding the meeting. However, notwithstanding the dread of yellow fever and the natural disinclination which eastern people have of going south-west in the uncomfortable month of August, the attendance of members, as indicated by the register, was only thirty less than at the Dubuque meeting when distance was the only cause of their non-attendance.

While it cannot be denied that the heat was excessive during the week of the meeting, it must also be acknowledged that the health of the city was unusually good, and with the exception of two or three members who were overcome by the heat on the long excursion to Pilot Knob and Iron Mountain, I did not hear of any case of sickness among the members, and the experience of this, as well as of the preceding meeting at Nashville, is certainly not unfavorable to future meetings in the south-western states.

The kindness and special attentions of several members of the Local Committee, combined with the comfortable quarters at the Lindell Hotel and the airy rooms at the Washington University where the sessions were held, easily reached by a few minutes ride in the horse cars, were great alleviators of the discomfort occasioned by the heat.



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The statistics of the meeting are as follows: — All the officers elect, excepting the Chairman of the Subsection of Microscopy and the Treasurer of the Association were present, as was also the retiring President. One hundred and thirty-four old and new members signed the register.

Two hundred and eighteen persons were elected to membership, including one hundred and seven citizens of St. Louis who, as members of the Local Committee, were, out of compliment, offered membership without the usual formalities. Of the whole number one hundred and seven, including thirty-three of the Local Committee, have perfected their membership. Up to this date five hundred and fifty-eight members have paid the assessment for the St. Louis meeting. Sixteen members were made fellows.

Notices of the decease of twelve members, including one past president and three of the founders of the Association, were given at the meeting.

One hundred and three papers were entered on the list, of which eight were declined either for want of abstracts or as not being appropriate to the Association, four were withdrawn by their authors, and one by Mr. Marshall was given as a free, popular lecture to the citizens of St. Louis. Of the papers placed on the daily programme for reading, thirty-five were in SECTION A, including eight in the subsection of Chemistry and two in that of Microscopy. Fifty-five were in SECTION B, divided as follows: Anthropology, eighteen; Geology and Paleontology, seventeen; Zoology, sixteen: Botany, four. Of the papers read in the two sections, fifty-three were accepted for publication in full or by abstract, but the authors of several of the accepted papers failed to send their manuscripts in time for publication in the volume, though most of these have appeared or will appear in other publications.

On Wednesday evening the addresses of Vice Presidents Thurston and Grote were delivered at a union meeting of *Sections* A and B. On Thursday morning the Subsection of Chemistry united with Section A, and the address of its Chairman, Prof. Clarke, was given. In the evening the retiring President, Prof. Newcomb, delivered his address in general session.

The Subsection of Chemistry held distinct sessions on Friday and Monday, and the Subsection of Microscopy had a conversazione on Friday afternoon and a session for the reading of papers on Tuesday.

Notwithstanding the fact that one-third of the papers entered in Section B related to Anthropology, it was deemed advisable by the Section not to sub-divide, and hence the Subsection of Anthropology did not officially organize.

The Entomological Club of the Association was well attended and held several meetings. At the meeting on Tuesday afternoon the President's address was delivered. The following Officers of the Club were elected for the next year: J. A. Lintner, of Albany, *President;* Wm. Saunders, of London, Ontario, *Vice President;* B. Pickman Mann, of Cambridge, *Secretary.*

The Cash Account presented to the Standing Commiteee at St. Louis, and covering the period embraced between August 12, 1877 and August 13,

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1878, including the Nashville meeting, presented a more favorable appearance than for several years previous, as will be seen by the account as printed on another page. Agreeably to a vote of the Standing Committee at the Nashville meeting the Treasurer has paid the debt due the Permanent Secretary, and the current receipts for the year were sufficient to meet the expenses for the same period, while, owing principally to the second generous donation from Professor William B. Rogers, a balance of one hundred and six dollars and seventy-six cents was carried over to the next account. The increase of receipts from entrance fees and assessments was about four hundred dollars over the preceding year, that from the sale of publications was a little over ninety dollars more, while the office and general expenses were one hundred and thirty-four dollars less than the year preceding. On the other side, the expense of the Nashville volume was one hundred and twenty-five more than the Buffalo volume. There was paid also a bill of twenty-five dollars for illustrations belonging to the Buffalo volume, and one of thirty-three dollars which should, following the custom of preceding years, properly be charged to expenses of the St. Louis meeting Fifty dollars were added to the Life Membership fund.

One thousand copies of the Nashville volume were received from the printer on July 19, 1878. Of these 737 have been distributed to members up to this date, 170 have been distributed by vote of the Standing Committee and 15 have been sold. Of back volumes 31 have been distributed to members, 115 by vote of Standing Committee, and 69 have been sold. Of the Memoirs, 7 copies have been sold, and 3 have been sent by vote of Standing Committee.

As the full report of the General Secretary gives the details in relation to the meeting, it is only necessary to allude here in general terms to the special courtesies which were extended to the members of the Association by the citizens through the Local Committee, and the facilities and concessions which were obtained by the Committee. First in order it is proper to acknowledge the efforts which the Local Committee made in regard to the transportation of members from all parts of the country at reduced rates of fare. In this work of love Dr. Taussig, the Chairman of the Local Committee on Transportation, was indefatigable, and though some of the railroad companies considered it necessary to put so many restrictions and regulations on their offers of reduced rates as to make it almost impossible for the members of the Association to take advantage of the concessions, others were far more liberal, and over many of the routes the way of the poor scientist was made easy. The proprietors of the several lines of sleeping and drawing room cars also were liberal in their concessions, and in general allowed members to return from St. Louis free of charge for berths and seats. The principal express companies of the country were not behind in liberality, and generously transmitted packages for members to and from St. Louis free of all expense. While the St. Louis Bridge and Transfer Companies were equally liberal and furnished free transportation on the presentation of a member's

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Association ticket, the Transportation Company also provided special coaches for the Association on several occasions during the meeting. To Col. Clowry, the Superintendent of the Western Union Telegraph Company, the members of the Association were under especial obligation for the arrangements made by which the Western Union permitted the free use of their lines throughout the country to members of the Association during the week of the meeting for all personal dispatches and for business relating to the Association. To members from a distance this was indeed a great concession on the part of the Telegraph Company, and one that was thoroughly appreciated.

As the proprietors of the principal hotels in the city also made a considerable reduction in their rates in favor of members of the Association, it will be realized that the expenses of members attending the meeting were much lightened.

To W. H. Pulsifer, Esq., the Chairman of the Local Executive Committee, and to Prof. J. K. Rees, the Local Secretary, too much praise cannot be given for their untiring efforts before and during the meeting to make the meeting a success, and in this work they were most cordially assisted by other members of the Local Committee, and though it is difficult to mention particular names, where so many were doing their utmost to make the wheels of science run pleasantly among the by-ways of social life, it would not be doing justice to the history of this meeting should omission be made of the names of Judge Speck, George Bain, Esq., Dr. George J. Engelmann, and Dr. Enno Sander, the several chairmen of the sub-committees of the local organization, who, with so many others, were ever ready to do all in their power to add to the comfort of members by lavish hospitality and good will to all. To the Chairman of the Local Committee, Chancellor Eliot of the Washington University, the Association was indebted for efforts in the preliminary arrangements and for his influence in obtaining the use of the University rooms for the sessions, though he was unfortunately absent from town during the meeting.

To the younger members of the Association it was gratifying to find among the Local Committee the respected and venerable botanist, Dr. George Englemann, one of the founders of the Association, who has ever taken a deep interest in its work and retained his membership intact from the beginning. It is needless to say more than that his warm heart made all who loved science welcome to his home, alas, since the meeting so sadly bereaved.

On Wednesday night, after the addresses of Vice Presidents Thurston and Grote, the members were conveyed to Schnaider's Garden, where they were invited to a Garden Concert, by the united singers of St. Louis, under the direction of Mr. Hans Balatka. Two hours were here most agreeably passed listening to several beautiful and stirring German songs by a large chorus, amid a quiet and orderly audience of several thousand persons.

On the following evening, after President Newcomb's address, the Citi-

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zen's Committee received the members of the Association at the parlors of the Lindell Hotel, and the opportunity was thus afforded members to make the personal acquaintance of many of the kind citizens of St. Louis.

Friday afternoon and evening were devoted to a ride in open carriages under the direction of the Reception Committee. Nearly all the members took advantage of this opportunity to see some of the beautiful portions of the great city of the West, and the pleasant drive was much enjoyed. The cool shade of the roads through Lafayette and Tower Grove Parks, was a most welcome change from the heat of the morning, and the drive and ramble through the far-famed, extensive and beautiful Park and Botanical Gardens of Mr. Shaw, were a delightful termination of the long drive which every one was fully capable of appreciating, while the sight of the hundreds of choice and rare plants of many orders, which were pointed out by Mr. Shaw and Dr. Englemann, must have been to the botanists a treat not often enjoyed.

Saturday was given to an excursion to Iron Mountain and Pilot Knob. A special train of eight cars containing three hundred and fifty members of the Association and invited guests, including many ladies, left St. Louis at quarter of ten in the morning, and all returned safe at midnight, after a profitable and enjoyable excursion, notwithstanding that the day proved the hottest during the meeting, the mercury indicating 98° in the cars. This excursion was suggested by Hon. Thomas Allen, President of the Iron Mountain and Southern Railroad Company, who furnished the train free of cost, and was under the personal charge of Mr. Bain, the Chairman of the Local Committee on Excursions, assisted by Messrs. Pulsifer, Speck, Col. Arthur, Manager of the road, and A. W. Soper, Esq, its Superintendent; and the successful management of the excursion of eighty-nine miles and safe return of the large party, is due to their great care and foresight. At Iron Mountain, the party were under the guidance of Prof. Broadhead, the State Geologist of Missouri, who gave an account of this interesting geological region. After the examination of the mountain and the extensive iron works upon it, the party went on to Arcadia, where a dinner was served under a large pavilion on the lawn of the hotel, after which the visit to Pilot Knob was made, and geology and history studied under able instructors, as there were present not only geologists who were familiar with the locality, but also both Union and Confederate officers who took part in the terrible battle of Pilot Knob in 1864. A writer who was on this excursion closes his account in the following words: "The trip back to the city was delightful. The soft evening air tempered the intense heat of the day, the lightning gleamed fitfully along a bank of clouds low on the horizon, the moon shed a silvery light on the passing scenery, and at midnight we were again safe in our quarters, after a day of absorbing interest and enjoyment."

On Sunday evening a steamboat was placed at the disposal of such of the members as wished to enjoy the cool evening breeze upon the river.

Monday and Tuesday were entirely given to work in the sections, and on Tuesday evening, the last general session was held.

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Preceding the adjournment announcements had been made by the Local Committee of the several excursions which were planned for Wednesday, and arrangements which had been made for the final excursion to Colorado. One of the trips planned for Wednesday morning, and enjoyed by a number of members was to the Silver Reduction Works at Cheltenham. Another excursion was to Cahokia Mound on the Illinois side of the river. This is the largest of the ancient mounds within the limits of the United States, and is located in the centre of a large group of mounds on the Great American Bottom. It is a pyramidal structure, now over ninety feet in height, covering an area of over thirteen acres, and proved a most interesting object to the large party that went on the excursion. Several members started in the morning in carriages under the guidance of Messrs. Conant, Hilder, Croswell and Patrick, and made a careful examination of the mound and its surroundings, and had a more satisfactory visit than fell to the lot of the large party that arrived by special train in the afternoon, but all were struck with wonderment at the sight of the immense tumulus, the work of an unknown prehistoric nation which once extended from the waters of the Mississippi eastward to the Alleghanies, and probably southward to the Gulf of Mexico.

At nine o'clock on Wednesday night the final departure of members from the city was begun, by the leaving of those who had made their arrangements for the Colorado excursion, which was to be of twelve days duration. This extensive excursion had been arranged with the utmost care by Dr. Taussig of the Local Committee, and owing to this not a delay nor an unpleasant incident occurred, and all who participated were warm in their praises of the arrangements which enabled them to accomplish and see so much in the time alloted, and at the moderate expense of not one-third the ordinary rates. The party consisted of forty-five, including several members of the Local Committee who took special charge of the excursionists and looked after all details. Two Pullman cars were placed at the disposal of the party, and Mr. V. W. Fisher, of St. Louis, with Mr. Lyman McCarty, of Kansas City, were the efficient managers *en route*.

It would be beyond the scope of these remarks to give a detailed account of this instructive and enjoyable excursion, which was regarded by all its participants as a most delightful closing of the twenty-seventh meeting of the Association.

F. W. PUTNAM,

SALEM, May 6, 1879. *Permanent Secretary.*

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## REPORT OF THE GENERAL SECRETARY.

THE ASSOCIATION assembled in the chapel of Washington University, St. Louis, Mo., at 10.30 A. M., on Wednesday, August 21, 1878, and was called to order by Prof. SIMON NEWCOMB, the retiring President, who introduced the President elect, Prof. O. C. MARSH. President MARSH accepted the chair in the following words: —

Members of the Association: I thank you most heartily for the honor you have done me in selecting me to preside over your deliberations. The Association in the past has held high rank among scientific organizations, and it will be my aim and my pleasure, with your aid, to see that this standard of excellence is in no degree diminished.

After prayer by the Rev. Dr. S. SONNENSCHEIN, Prof. W. T. HARRIS, superintendent of public schools and chairman of the citizens reception committee, addressed the Association as follows: —

MEMBERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE: In behalf of the local committee, partly representing the St. Louis Academy of Science and partly representing the hospitality and public spirit of our citizens at large, it has been made my pleasant duty to introduce his Honor, the Mayor of St. Louis, who will extend to you the city's formal welcome. And by way of preface to that welcome which comes to you from our body politic — our municipal corporation — I am charged to express to you the gratification which is felt on this occasion by the members of our Academy of Science, and shared by all of our citizens who love the pursuit of science itself, or who appreciate its useful applications to productive industry. Only once before during the thirty years of your existence as the most honored and most useful of American societies, have you held your annual meeting on the west bank of the Mississippi.

Since your foundation in 1847, the centre of population in the United States has moved westward nearly two hundred miles, and the population now living west of the Mississippi is nearly ten times what it was then. By this we are strengthened in our hope that your future meetings will oftener be held in the west. For we have all read the noble words in which your constitution expresses the objects of the Association: "By periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of America; to give a stronger and more general impulse and more systematic direction to scientific research; and to procure for the labors of scientific men increased facilities and a wider usefulness."

That the objects of the Association have been realized in the past, and are destined to be to a still greater degree realized in the future, is the occasion for the congratulations which we now offer you, as well as for

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those which we exchange among ourselves over the auspicious event of your presence with us to-day. That the scientific mind of our whole country shall, as it were, focus itself in turn upon each special department of research as it is presented here in your deliberations, is of untold value in the way of stimulating investigation and of guiding aright its application. That, on the other hand, each specialist here participates in the fruits of the labors of all specialists, is of still greater value in broadening the views, in correcting methods and in giving that true scientific insight which comes only to those who study each thing in the light of its relations to all other things. To your meetings and the meetings of your British namesake (only sixteen years older than your own society) is chiefly due the fact, that science, even pure science, is so highly respected by the so-called "practical business men" of our time. The twenty-six volumes of your annual proceedings, and the similar volumes that record the transactions of the British Association for the Advancement of Science, contain a complete record of scientific discovery during the period in which the greater part of our knowledge of nature has been accumulated. Step by step, with this progress of science, the useful arts have unfolded. The victories of science, which have revealed to us the structure of Nature and her fixed laws, have been succeeded by a conquest over the forces of Nature and their subjection to man's service. This has compelled men to recognize the value of natural science, and it is no longer considered visionary to occupy one's self with the study of the departments of Nature apparently remote from human interests and industrial uses.

In the recognition and appreciation of the fruits of science, an impelling cause is to be found in the widespread evils which oppose the unaided might of man, and cover his labors with disaster. In the West you will find eager listeners everywhere to your discussions of the questions of meteorology, for there is the tornado and the drought which we would gladly prevent or avoid. Your entomologists will be highly in honor, because we have the potato bug and the weevil, and the destroyers of the vine and of the fruit trees, and the famine-producing locust among us. Your subsection of microscopy will be attended by people anxious to hear of the discovery of the animal or vegetable growths that cause the contagion of our various pestilential diseases.

A more general interest in your investigations will be manifested by our merchants, manufacturers and business men engaged in mining or in transportation, for they see in the wake of science the luminous trail of inventions which annihilate for us those vast distances in space which baffle our western energy — inventions which convert our raw ores into metals, then into manufactured products for the markets of the world. These men will tell you with pride of the dreams which they dream of the future greatness of Missouri, its location on the great rivers just at the focus of collection and distribution of northern productions for the south and of southern productions for the north — at the point where freight must be broken and made up anew — a state extending north to the middle

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latitudes of Pennsylvania, Ohio and Illinois, and south to the middle latitudes of Tennessee and North Carolina, occupying a position, as regards the far west, somewhat similar to that of the Keystone State, Pennsylvania, which, with the settlement of the Ohio valley, became the greatest manufacturing state in the Union, because of its coal and iron, and its commanding position. We of Missouri are not a little vain of our possession of iron, coal and lead, and of our holding the key position as regards the southwest, and hence it happens that in the midst of our actual poverty of realization — our wealth and greatness being mostly possibilities — we find a never-failing source of consolation in making these inventories of natural advantages which even our enemies concede to us. With good reason our people prize science; for its fruits in this state will surpass all its harvests in other times and other fields.

But, in behalf of our St. Louis Academy of Sciences, allow me to welcome you here, especially on the ground that your Association gives encouragement to science for its own sake and not for its directly practical results in the fields of productive industry. They are aware, it is true, that the disinterested pursuit of scientific truth is resultant in the discovery of what is sure to prove most useful to art. But they well know that the scientific man whose eyes look askance at the useful, while he is in the pursuit of truth, will fail in his labors because of the distraction of his mind. To your visit, therefore, our Academy looks for great assistance in the form of an increased interest among our citizens in natural science, which will continue to manifest itself after your departure.

Trusting that you will appreciate the earnestness and sincerity of the welcome extended to your illustrious assemblage, all the more from this recapitulation of the interested motives which have their weight with individual citizens among us, I beg here to introduce the bearer of our city's welcome, the chief magistrate of St. Louis, his Honor, Mayor Overstolz.

His Honor, Mayor Henry Overstolz, on behalf of the city of St. Louis, delivered the following address of welcome: —

Mr. President and Gentlemen: The meeting in St. Louis of so distinguished a body as the American Association for the Advancement of Science is an event deeply interesting to our people, and in their name and as chief officer of the Municipal Government I extend to you all a sincere and cordial welcome. All that can be done to render your visit agreeable and to promote your entertainment will be attended to by the local committees, and it is our hope that the pleasant incidents of your reception, the hospitalities of our citizens, the interest and importance of your proceedings, will make the occasion memorable in the history of your Association, and enable each member to appreciate properly the spirit and culture of our people, the delightful characteristics of St. Louis as a place of residence, and its true commercial influence and importance.

This city, gentlemen, is essentially an industrial one — the centre and the capital of a vast agricultural and mineral region; our energies and

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efforts are naturally devoted to business, to trade, to manufactures, to all kinds of commercial enterprises. The majority of our people have not much time for scientific investigations and the higher intellectual studies, but our schools and universities, our libraries and other institutions of learning, will convince you that we are in active and earnest accord with the objects of your Association. We are well aware that the advancement of true science means the advancement of civilization, that it means progress in every branch of thought and of practical industry, that it means new light and knowledge to all classes of society, that it means toleration in all matters of opinion, the wisest method and principles in all matters of practice, and the correction of errors, without controversy or contention, by the discovery and development of truth, that it means in a word the advancement and happiness of the race. Your Association has in view these lofty objects, so vitally connected with true national prosperity, and for this reason we consider the meeting of your Association here as an honor to the city, and will regard your deliberations with profound and respectful interest. To the Association as a body and to the many gentlemen I see present whose names are illustrious in the literature of science and philosophy, I have the honor to tender the hospitality and heartfelt welcome of the city of St. Louis. We have had many conventions in this city during past years, commercial, political and religious; but this one for the advancement of science — this one that stands apart from all the antagonisms and hot issues of the hour, which has to do only with the simple and majestic cause of truth, possesses to us a peculiar and universal significance. All classes unite in the good wishes I have expressed, and the heart of our population inspires the welcome I now extend.

President Marsh replied as follows: —

In behalf of the Association, I return our best thanks to the gentlemen who have spoken, and through them to the people of St. Louis, for this most cordial welcome. It is fitting that the Association, after having met in various smaller cities in the east and the west, should at last come to the great metropolis of the Mississippi Valley; and it is especially gratifying to receive here such a greeting. In thus honoring science, St. Louis honors itself. We come here not among strangers; because you have in your midst two institutions that are known and respected among scientific men throughout the country. The St. Louis Academy of Science and the Washington University, however quietly their work is done, do great credit to the city of St. Louis and the state of Missouri. This vast state of Missouri with its enormous natural resources can afford to be generous to science, and especially to cherish these two institutions. Science has done much for this community, in developing directly and indirectly its resources, and the debt, thus incurred, should be paid at home.

We trust that the present meeting will do much for the Advancement of Science here. In conversing with one of the leading scientific men of England recently, he said that the work of the British Association for the

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Advancement of Science was nearly completed, while that of the American Association was just beginning.

We extend a cordial invitation to the people of St. Louis to attend the meetings during our session, and will endeavor to repay them in some measure for this generous reception.

THE GENERAL SECRETARY, DR. H. CARRINGTON BOLTON, then announced the arrangements made by the local and standing committees, as to the rooms for the several sections and subsections of the Association.

He also read the following letter from W. H. PULSIFER, Esq., Chairman of the Executive (Local) Committee: —

UNIVERSITY CLUB, ST. LOUIS, AUGUST 20, 1878.

TO PROF. F. W. PUTNAM,

*Permanent Secretary, A. A. A. S.*

SIR: — The Local Committee respectfully invite the members of the American Association for the Advancement of Science to participate in the following excursions and entertainments during the session of the Association in the city.

On Wednesday, August 21st, the Association is invited through the politeness of Mr. HANS BALATKA, representing some of the principal musical societies of the city, to attend an open-air concert to be given at SCHNAIDER'S GARDEN on Chouteau and Mississippi Avenues. For the convenience of members, omnibuses will leave the Lindell Hotel at 8.30 P. M. The grounds can also be reached by street cars.

A reception at the parlors of the LINDELL HOTEL, to occur immediately after the address of the retiring President, is tendered.

A special train has been offered by Hon. THOMAS ALLEN, President of the St. Louis, Iron Mountain and Southern Railway, to convey members and their ladies to IRON MOUNTAIN and PILOT KNOB. Omnibuses will leave the Lindell Hotel on Saturday morning, Aug. 24, at 9 o'clock. The train will leave Plum St. Depot at 9.30. Returning, the city will be reached at 9 P. M.

On Monday afternoon carriages will be provided for a drive through LAFAYETTE and TOWER GROVE PARKS, the ZOÖLOGICAL and SHAW'S BOTANICAL GARDENS.

Arrangements have been made for an excursion on Wednesday, Aug. 28th, to the SILVER REDUCTION WORKS, at Cheltenham, four miles from town. Trains will leave at 9 A. M., returning about noon.

At 2 P. M. of same day, a special train will leave the Main St. Depot for a visit to CAHOKIA MOUND.

Arrangements have been made for opening, at this time, a mound which it is believed has been undisturbed since its erection.

Respectfully yours,

W. H. PULSIFER,

*Chairman Executive Committee.*

On motion, the reception at the Lindell Hotel and the excursion to Iron

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Mountain were accepted. The other entertainments were also accepted after slight modifications respecting the hours.

The GENERAL SECRETARY then read the following communication from Mr. GEORGE BAIN, President of the Merchants' Exchange: —

ST. LOUIS, AUGUST 20, 1878.

PROF. F. W. PUTNAM,

*Permanent Secretary, A. A. A. S.*

DEAR SIR: — I take pleasure in extending a cordial invitation to the members of the "American Association for the Advancement of Science," to visit the MERCHANTS' EXCHANGE, at such time as may best suit their convenience. The regular session of the Exchange is from 11 A. M. to 1 P. M., but the rooms will be open at such time as you may wish to visit them.

Very truly yours, etc.,

GEORGE BAIN,

*President.*

A meeting of the Entomological Club was announced for 3.30 P. M., at room No. 17 of the Lindell Hotel.

Announcement was made that the titles of sixty papers had been received to date.

The Association then proceeded to the election by ballot of six Fellows for members of the Standing Committee; the following gentlemen were elected, by ballot, from the nominees: —

PROF. A. G. WETHERBY, of Cincinnati.
PROF. C. V. RILEY, of Washington.
PROF. F. W. CLARKE, of Cincinnati.
DR. GEORGE ENGELMANN, of St. Louis.
MR. J. W. OSBORNE, of Washington.
PROF. A. H. WORTHEN, of Springfield.

The GENERAL SECRETARY read the names of thirty persons recommended to membership by the Standing Committee and they were unanimously elected.

The PERMANENT SECRETARY, PROF. F. W. PUTNAM, presented the following brief notices of deceased Members: —

News of the deaths of eleven members of the Association has been received since the Nashville meeting. This list includes three of its original members and one of its past presidents.

Dr. ABRAM SAGER, of the University of Michigan, died in 1877. He was elected a member of the Association at its 6th meeting.

Mr. EZRA READ, of Terre Haute, Ind., died in 1877. He joined the Association at the 20th meeting.

Prof. JOSEPH HENRY, of Washington, D. C., died in Washington, May 13, 1878. He was an original member of the Association, and President of the 2nd meeting, held in Cambridge, Mass., in 1849, and had always taken an active interest in the Association.

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Prof. C. F. HARTT, of Cornell University, and Director of the Geological Survey of Brazil, died in Rio Janeiro, in March, 1878. He joined at the 18th meeting, and was elected General Secretary for the following meeting but, owing to his being in Brazil, did not serve.

Mr. JAMES SWAIN, of Fort Dodge, Iowa, died December 27, 1877. He joined the Association at the 21st meeting, and was one of the active members at the Dubuque meeting.

Mr. G. W. KEELY, of Waterville, Maine, died in 1878. He was an original member of the Association.

Hon. JOHN V. L. PRUYN, of Albany, New York, died in 1878. He, also, was an original member of the Association.

Mr. JAMES BOWRON, of South Pittsburg, Tenn., died in December, 1877. He joined the Association at the 26th meeting.

Mr. WILLIAM MONROE, of Concord, Mass., died April 27, 1877. He joined at the 18th meeting.

Rev. JOHN W. ARMSTRONG, D. D., of Fredonia, N. Y., died August 12, 1878. He joined at the 24th meeting.

Prof. EBENEZER S. SNELL, of Amherst, Mass., died in 1877. He joined at the 2nd meeting.

Prof. F. W. BARDWELL, of Lawrence, Kan., died in August, 1878. He joined at the 13th meeting.

The recommendation of the STANDING COMMITTEE that the daily sessions should begin at 10 A. M. and continue until 2.30 P. M., with a half hour's recess at noon, was adopted.

The Association then adjourned to meet in sections.

*Section A* met in the Chapel of Washington University and was called to order by *Vice President*, Prof. ROBERT H. THURSTON, of Hoboken; the *Secretary*, Prof. F. E. NIPHER, of St. Louis, being in attendance. The following gentlemen were elected members of the Sectional Committee:—

DR. R. H. WARD, of Troy.
PROF. SIMON NEWCOMB, of Washington.
DR. H. C. BOLTON, of Hartford.

*Section B* met in Room No. 10, Library of Washington University, and was called to order by *Vice President*, AUG. R. GROTE, of Buffalo, the *Secretary*, Dr. GEORGE LITTLE, of Atlanta, in attendance. The following members were chosen for a Sectional Committee:—

HON. LEWIS H. MORGAN, of Rochester.
PROF. J. M. SAFFORD, of Nashville.
PROF. J. A. LINTNER, of Albany.

The following were elected members of the Nominating Committee:—

PROF. J. G. HENDERSON, of Winchester.
PROF. G. C. BROADHEAD, of Columbia.
DR. E. A. DALRYMPLE, of Baltimore.
DR. W. LE ROY BROUN, of Nashville.



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The *Permanent Subsection C* met in Room No. 2 of Washington University and was called to order by Prof. F. W. CLARKE, of Cincinnati, *Chairman*. The section organized by electing A. P. S. STUART, *Secretary*, and adjourned.

The *Permanent Subsection D* organized in Room No. 4 of Washington University. In the absence of the *Chairman*, Dr. GEO. S. BLACKIE, of Nashville, Dr. R. H. WARD, of Troy, was elected *Chairman pro tem.*, and Dr. GEO. J. ENGELMANN, of St. Louis, was elected *Secretary*.

By reason of the small attendance of members it was determined to meet for the present with *Section A*.

At 7.30 P. M. the addresses of Vice Presidents THURSTON and GROTE were delivered before the Association in General Session at the Chapel of the University.

*Thursday, August 22nd.* The Second Day's Session was opened in the Chapel of the University at 10.15 A. M., President MARSH in the Chair.

The GENERAL SECRETARY read the following letter from the Superintendent of the Mercantile Library: —

MERCANTILE LIBRARY, ST. LOUIS, AUG. 21ST, 1878.

PROF. J. K. REES,

*Local Secretary of the A. A. A. S.*

By order of the Board of Directors, I am instructed to extend a cordial invitation to the members of your Association to make use of the reading room and library during the session of your convention. On registry of names at the library, the librarian will be pleased to show attention to the members.

Very respectfully,

JNO. N. DYER,

per W. H. H. ANDERSON, *Asst. Librarian.*

The Report of the Committee to memorialize Congress and State Legislatures regarding the Cultivation of Timber and the Preservation of Forests was presented, through its Chairman, FRANKLIN B. HOUGH, and accepted.

The names of thirty-six persons recommended to membership by the STANDING COMMITTEE were read, and the persons were elected.

PRESIDENT MARSH announced that the retiring President, Prof. SIMON NEWCOMB would deliver his address at 7.30 P. M., in the Chapel of the University.

The meeting then adjourned to meet in sections.

In *Section A* the following gentlemen were chosen for members of the Nominating Committee: —

PROF. JOHN K. REES, of St. Louis.
PROF. PAUL SCHWEITZER, of Columbia.
PROF. GEORGE F. BARKER, of Philadelphia.
MR. ARTHUR H. ELLIOTT, of New York.

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At 8 P. M. the address of the retiring President, Prof. SIMON NEWCOMB, of Washington, was delivered in General Session at the Chapel of the University.

*Friday, August 23rd.* The Third Day's Session was opened at 10.15 A. M. in the Chapel of the University, President MARSH in the Chair.

The GENERAL SECRETARY made the following announcements:—

Newly elected and newly arrived members are requested to register their names at the office of the PERMANENT SECRETARY on the first floor.

Members intending to take part in the excursion to Iron Mountain are informed that conveyances will leave the Lindell Hotel at 9 A. M. on Saturday.

Members proposing to join the Colorado Excursion can obtain tickets of Dr. Wm. Taussig, at Room No. 22, Lindell Hotel, from 9 to 10 A. M. and from 5 to 6 P. M.

The STANDING COMMITTEE recommend to the Association alterations in Article 35 of the Constitution, so that it read:—

"Any member or fellow who shall pay the sum of fifty dollars to the Association at any one time shall become a Life Member, and as such shall be exempt from all further assessments, and shall be entitled to the Proceedings of the Association. All money thus received shall be invested as a permanent fund, the income of which shall be used only to assist in original research unless otherwise directed by unanimous vote of the Standing Committee."

In accordance with Art. 38 of the Constitution, this recommendation lies over until the next annual meeting of the Association.

The STANDING COMMITTEE recommend that the Committee to Memorialize Congress in relation to Meteorological Researches be continued, that Prof. ELIAS LOOMIS, of New Haven, be requested to act as Chairman, and also that Mr. J. W. OSBORNE be appointed to fill the vacancy caused by the resignation of Dr. JAMES PLUNKET. Adopted.

The PERMANENT SECRETARY announced a second donation of one hundred dollars from Past-president, Prof. WILLIAM B. ROGERS, of Boston.

The GENERAL SECRETARY thereupon announced the following recommendation of the STANDING COMMITTEE:—

*Resolved*, That Prof. WILLIAM B. ROGERS' name be placed on the list of Life Members of the A. A. A. S.

The resolution was unanimously adopted.

Attention was called to Dr. B. F. SHUMARD'S collection of Fossils and Minerals on exhibition daily from 2 to 5 P. M., at northeast corner of Pine and 16th Streets.

On motion of Vice President GROTE, in behalf of the Committee, the Committee on the Introduction of Science into the Schools was continued, and Profs. E. W. HILGARD, of Berkeley, and GEO. LITTLE, of Atlanta, were added to the Committee.

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On motion, a resolution was adopted permitting Mr. THOMAS BASSNETT, of Jacksonville, to read a brief communication already referred to the STANDING COMMITTEE.

Whereupon Mr. BASSNETT presented the following:—

influence in the ordinary business of the world, and by their close relation to many of the most interesting problems in Physics.

THOS. BASSNETT.

Prof. GEO. F. BARKER, of Philadelphia, then addressed the Chair, and in a few appropriate words introduced to the Association his friend, Dr. THOMAS A. EDISON.

PRESIDENT MARSH welcomed Dr. Edison as a man who had done great service to science by his wonderful inventions, and who had reflected much honor upon the inventive genius of America. He was glad to be able to announce to Dr. Edison and the members that the Association had that morning received a telegram from Paris stating that our distinguished guest had been awarded the grand prize at the Exposition for one of the most wonderful inventions of the age.

The PRESIDENT appointed the following gentlemen a committee to prepare a Eulogy on the late JOSEPH HENRY, a Past President of the Association:

PROF. SIMON NEWCOMB, of Washington.
PROF. S. F. BAIRD, of Washington.
PROF. A. M. MAYER, of Hoboken.

The GENERAL SECRETARY read the following communication:—

ST. LOUIS, AUG. 22ND, 1878.

F. W. PUTNAM, ESQ.,

*Permanent Secretary, A. A. A. S.*

DEAR SIR:—We take pleasure in extending to the members of the American Association for the Advancement of Science a cordial invitation to avail themselves of the privileges of the St. Louis Public School Library and Reading Room.

It may be of interest to announce in this connection that the library of the St. Louis Academy of Science is in our custody.

Very Respectfully,

T. B. RODGERS,

FRED. M. CRUNDEN, *Librarian.* *Vice President.*

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Announcement was made that the titles of ninety-one papers had been received to date.

Seventeen persons were then nominated by recommendation of the STANDING COMMITTEE, and duly elected.

The General Session then adjourned to meet in sections.

*Monday, August 26th.* The Fourth Day's Session was held in the Chapel of the University, beginning at 10.15 A. M., PRESIDENT MARSH in the Chair.

The GENERAL SECRETARY read the following recommendation of the STANDING COMMITTEE:—

*Resolved*, That a complete set of the Proceedings of the Association be presented to the Library of Washington University, also that the Academy of Sciences, the Public School and the Mercantile Libraries be furnished with missing volumes of the Proceedings so far as possible.

Adopted.

The PRESIDENT presented an informal report on behalf of the Committee to memorialize Congress in relation to the Yellowstone National Park. On motion the Committee was continued and Mr. CLARENCE KING was appointed to fill the vacancy in the Committee.

The PRESIDENT also appointed Mr. J. W. OSBORNE to fill the vacancy in the Committee on the relation of Science to the Industrial Arts.

Prof. ROBERT H. THURSTON, Chairman of said Committee, read the following communication from Mr. O. CHANUTE:—

NEW YORK, AUGUST 17, 1878.

R. H. THURSTON, ESQ.,

DEAR SIR:—I have received your favor of 6th inst., stating that I am a member of a committee of the American Association for the Advancement of Science (of which you are Chairman), "*On the relations of Science to the Industrial Arts*," and asking me to furnish such paper or memorandum falling within the scope of the resolution as I may deem desirable to present.

I regret not to have known sooner that I had been appointed on this committee, in order that I might have had more time to think of the subject.

Only one suggestion now occurs to me. It may conveniently be divided under three heads:—

1. That the committee invite, by circular, communications from leading manufacturers, heads of public works, managers of mines, engineers, agriculturists, or business men, concerning the scientific problems or questions which may have arisen in the pursuit of their avocations, and suggestions as to the subjects or points upon which they may deem that there exists a lack of scientific investigation or elucidation.

2. That from this, or similar information otherwise obtained, the committee shall annually publish a list of subjects pertaining to the application

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of science to the industrial arts, upon which it shall invite papers from the members of the Association. I fancy that a list of this kind might be made at once by the committee, as a beginning for this year.

3. That either the committee or the Association issue an address to such of our wealthy men as are interested in science, suggesting the endowment of a number of prizes or medals, to be designated by the name of the donor, to be annually offered, under proper restrictions, to the authors of the best papers upon the particular class of subjects which they may designate. I believe that eight or ten endowments of $1000 to $2000 each, the revenue from which should be publicly bestowed in prizes, would prove a sufficient inducement to lead to the careful preparation of papers, and that the method of designating certain subjects as requiring investigation or elucidation may well prove a valuable supplement to the present system of inviting voluntary contributions.

Yours respectfully, O. CHANUTE.

After sundry announcements by the GENERAL SECRETARY relative to a drive through the Parks of the City, the Association proceeded to the election of Fellows recommended by the STANDING COMMITTEE.

The PRESIDENT appointed Messrs. MICHELSON and CHAUVENET tellers; ballots were cast, and teller Michelson announced that all the persons recommended had been elected, as follows:—

PROF. STEPHEN ALEXANDER, Princeton, N. J.
DR. EMIL BESSELS, Smithsonian Inst., Washington, D. C.
PROF. LEWIS BOSS, Director Dudley Observatory, Albany, N. Y.
WM. M. CANBY, Wilmington, Del.
DR. THOS. A. EDISON, Menlo Park, N. J.
PROF. CHAS. F. HASTINGS, Johns Hopkins Univ., Baltimore, Md.
DR. GEORGE J. ENGELMANN, St. Louis.
PROF. JOSEPH FICKLIN, Columbia, Mo.
PROF. WM. HARKNESS, U. S. Observatory, Washington, D. C.
CLARENCE KING, U. S. Geolog. Expl. 40th Parallel, New York.
PROF. BENJ. F. MUDGE, Manhattan, Kansas.
MRS. ELLEN S. RICHARDS, Mass. Inst. Technology, Boston, Mass.
PROF. JOHN K. REES, Washington University, St. Louis, Mo.
MISS EMILY A. SMITH, State Entomologist, Peoria, Ill.
PROF. J. E. WILLET, Mercer University, Macon, Ga.
DR. THEODORE G. WORMLEY, University of Pa., Phila., Pa.

The Association then elected 107 persons to membership, including all the members of the Local Committee, all formalities being waived in favor of the latter.

The PRESIDENT announced a meeting of the Standing Committee for 7.30 P. M. at Room No. 166, Lindell Hotel, and a meeting of the Nominating Committee at 8.30 P. M. in Room No. 22 in the same building.

The PRESIDENT announced the appointment of the following persons on

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the Committee to obtain Permanent Funds for the Association, and to advise with Treasurer and Permanent Secretary on financial matters: —

PROF. WILLIAM B. ROGERS, of Boston.
PROF. J. LAWRENCE SMITH, of Louisville.
HON. LEWIS H. MORGAN, of Rochester.
PROF. ALEXANDER AGASSIZ, of Cambridge.
PROF. ROBERT H. THURSTON, of Hoboken.

Adjourned to meet in sections.

*Tuesday, August 27th.* The Fifth Day's Session was opened at 10 A. M. in the Chapel of Washington University, President MARSH in the Chair.

The GENERAL SECRETARY stated that the Local Committee had requested him to announce, in reference to the excursion to Cahokia Mound the next afternoon, that those who intended participating in the Cahokia excursion were to assemble at the Lindell Hotel at 1.30 P. M., to be conveyed to the train which would start at 2 P. M. The party would return in ample time to take the Colorado excursion, which would leave at 9.20 P. M. at Union Depot. He also said that there had been much delay in giving names for the Colorado excursion, which Dr. Taussig desired rectified by prompt application to-day.

The following resolution, presented by Mr. E B. ELLIOTT and recommended by the STANDING COMMITTEE, was read by the GENERAL SECRETARY: —

*Resolved*, That the Congress of the United States be respectfully requested to take such steps as may seem advisable to secure a proper coöperation between the general government and the several state governments of the United States, to establish a common, efficient and uniform system for the registration of births, deaths and marriages.

Adopted.

Dr. H. C. BOLTON introduced the following resolution, recommended by the STANDING COMMITTEE: —

*Resolved*, That a committee of five be appointed by the Chair, to be styled a committee on membership, the President, General Secretary and Permanent Secretary to be members *ex-officio*. It shall be the duty of said Committee through systematic effort to increase the membership of the Association, by presenting to the Standing Committee the names of persons interested in the advancement of science.

President MARSH said that at present the question of membership was left largely to the care of personal friends, and members' names were generally presented only during the meeting, but by appointing a committee which can receive names during the whole year, from all quarters of America, it was believed that the efficiency of the Association might be increased, its numbers at the meetings augmented, and its financial basis secured.

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The resolution was adopted, and the Chair appointed the following gentlemen as the Committee:—

PROF. J. S. NEWBERRY, Columbia College, New York.
PROF. SIMON NEWCOMB, Washington.
PROF. WILLIAM B. ROGERS, Boston.
PROF. JAMES D. DANA, New Haven.
PROF. J. LAWRENCE SMITH, Louisville.

A member suggested that the appointment of the Committee and its object should be made public. President MARSH said the Committee would take the necessary steps to bring it before the public, and that it had already been suggested that circulars be issued.

The PERMANENT SECRETARY announced that a copy of "Pictorial St. Louis" had been presented to the Association by Mr. A. H. St. John. The letter of the donor was read, in which he said the Members of the Association, in glancing over its pages, would be reminded of a hotter place than Boston.

President MARSH accepted the gift on behalf of the Association, and said it would be fully appreciated.

President MARSH stated that the Nominating Committee had met on Monday evening, and after due consideration had unanimously reported the names of the following gentlemen for general officers of the Association for the ensuing year:—

*President*,— PROF. GEORGE F. BARKER, of Philadelphia.
*Vice President, Section A*,— PROF. S. P. LANGLEY, of Alleghany.
*Vice President, Section B*,— MAJOR J. W. POWELL, of Washington.
*General Secretary*,— DR. GEORGE LITTLE, of Atlanta.
*Secretary of Section A*,— PROF. JOHN K. REES, of St. Louis.
*Secretary of Section B*,— PROF. A. G. WETHERBY, of Cincinnati.
*Treasurer*,— WM. S. VAUX, of Philadelphia.
*Auditing Committee*,— DR. HENRY WHEATLAND, of Salem.
THOMAS MEEHAN, of Philadelphia.

Prof. MARSH said that the Permanent Secretary was elected for five years, and still had a year to serve; the election of that officer is to take place at the next meeting.

On motion, the report of the Committee was unanimously accepted, and President MARSH was authorized by the meeting to cast the ballot of the Association for the nominees, which he did, and they were declared elected.

The GENERAL SECRETARY announced that the following gentlemen were elected Chairmen of the respective subsections:—

*Chairman of Subsection of Chemistry*,— PROF. IRA REMSEN, of Baltimore.
*Chairman of Subsection of Microscopy*,— PROF. EDW. W. MORLEY, of Hudson, Ohio.

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President MARSH said that the Nominating Committee had given careful consideration to the question of the next place of meeting. They had received an enthusiastic invitation from Cincinnati. Invitations had also been received from St. Paul, San Francisco, Rochester and Boston. The Committee were, however, of the opinion that the next meeting should be held at Saratoga on the last Wednesday (27th) of August.

A vote being taken, the recommendation of the Committee was unanimously adopted.

The following resolution recommended by the STANDING COMMITTEE was adopted: —

*Resolved*, That the thanks of the Association are hereby tendered to the mayor, citizens and institutions of Cincinnati, for honoring the Association with several most cordial invitations to hold a meeting in that city, and that it entertains the hope of holding a meeting there at no distant day.

Thirteen additional persons were recommended by the Standing Committee and elected to membership in the Association.

Adjourned to meet at 8 P. M.

The *Evening Session* was opened at 8 o'clock, in the Chapel of the University, President MARSH in the Chair.

The GENERAL SECRETARY announced the arrangements for the excursion to the Silver Reduction Works at Cheltenham.

A communication was received and read from A. A. E. TAYLOR, Chairman of the Committee of entertainment of the Ohio State Archæological Association, announcing the meeting of the society at Worcester, Ohio, on the 3d, 4th, and 5th of September, and inviting members of the Scientific Association to attend.

Resolutions being in order, the following votes of thanks were introduced, and appropriate remarks made by the movers.

By Vice President ROBERT H. THURSTON: —

*Resolved*, That the thanks of the Association are hereby tendered to his Honor, the mayor, and other officers of the municipal government of St. Louis, for the hearty welcome and encouragement which we have received from them at this session of the American Association for the Advancement of Science.

By Prof. F. W. CLARKE: —

*Resolved*, That the thanks of the American Association for the Advancement of Science be extended to the Chancellor and Trustees of the Washington University for the use of the University buildings during the St. Louis meeting.

By Major J. W. POWELL: —

*Resolved*, That we heartily thank the citizens of St. Louis and their representatives on the Local Committee, and especially Messrs. W. H. Pulsifer, Judge Charles Speck, Dr. George J. Engelmann, W. T. Harris,

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George Bain, J. R. Shepley, Dr. Enno Sander, Judge H. W. Jones and Jno. N. Dyer, for their untiring exertion in behalf of the Association, both before and during its meetings.

By Dr. GEORGE LITTLE: —

*Resolved*, That our sincere thanks are due the St. Louis Academy of Sciences, for the courtesy and kindness shown us by its members during the present meetings; and that we hereby express our high appreciation of the value of their labors, not only to the city in which they live, but to the cause of science at large.

By Dr. GEO. F. BARKER: —

*Resolved*, That Dr. Wm. Taussig, of St. Louis, is especially entitled to our gratitude, for his unremitting labors as Chairman of the Transportation Committee; and that we hereby collectively and individually recognize the fact that to him we owe much of the success of our St. Louis meeting.

By Dr. R. H. WARD: —

*Resolved*, That we heartily thank Professor John K. Rees, Local Secretary, for the deep interest he has taken in the welfare of the Association, and for his untiring and successful efforts in its behalf.

By Mr. E. B. ELLIOTT: —

*Resolved*, That we hereby express our thanks to the Hon. Thomas Allen, President of the St. Louis, Iron Mt. and Southern R. R., for the use of the excursion train to Iron Mt. and Pilot Knob; and also to A. W. Soper, Esq., Sup't of the R. R., for his extreme kindness and excellent arrangements on the trip.

By Dr. E. A. DALRYMPLE:—

*Resolved*, That the cordial thanks of the Association be, and are hereby, returned to the officers of the Mercantile Library, and to the authorities of the Public School Library of St. Louis, for their consideration and courtesy in so liberally extending to our members the use of their Libraries and apartments during our meeting.

By Prof. C. V. RILEY:—

*Resolved*, That we warmly thank Mr. Henry Shaw for his kind attention in showing the Association the many beauties of his famous garden.

By Mr. J. W. OSBORNE:—

*Resolved*, That we hereby express our thanks to Eben Richards, Esq., for his kindness and care in arranging our excursion on the river, and our visit to the St. Louis Water Works.

By Prof. F. W. PUTNAM:—

*Resolved*, That in anticipation of an interesting excursion tomorrow,

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we tender our thanks to the Vandalia R. R. and the St. Louis Bridge and Transfer Companies, for their offer to convey those of us who wish to join the excursion to and from Cahokia Mound, and to Messrs. A. J. Conant, F. F. Hilder, and other members of the Local Committee, for the arrangements made, enabling us to visit the largest of the ancient mounds within the limits of the United States.

By Mr. THOS. BASSNETT:—

*Resolved*, That our sincere thanks are due to Col. R. C. Clowry, Supt. of the Western Union Telegraph Co., for his generous services to the Association in connection with the telegraph.

By Prof. J. M. SAFFORD:—

*Resolved*, That a vote of thanks be extended to the St. Louis Transfer Company for its very great liberality towards the members of the Association.

By Dr. J. R. WALKER:—

*Resolved*, That the Adams, American, United States, Union Pacific, and Wells' and Fargo's Express Companies, are all entitled to our gratitude for the assistance they have rendered to our members in the free transportation of apparatus and specimens.

By Mr. ARTHUR H. ELLIOTT:—

*Resolved*, That we are deeply indebted to the managers of the Silver Reduction Works, and to the Missouri Pacific R. R., for their kindness in arranging an excursion to the said works.

By Prof. ISAAC E. HASBROUCK:—

*Resolved*, That this Association hereby tenders hearty thanks to the Pullman, Woodruff and Wagner Sleeping Car Companies, for their very great liberality in affording accommodations to members of the Association.

Mr. J. S. PARVIN, of Iowa City, made some remarks thanking the Association for coming to St. Louis.

Prof. ROBERT H. THURSTON presented a resolution of thanks to the gentlemen of the Press.

Mr. W. H. H. RUSSEL and Dr. WALKER made additional remarks.

President MARSH then congratulated the people of St. Louis on the success of the meeting, which had depended largely on their harmonious coöperation, and declared the Association adjourned until 10 o'clock A. M., August 27, 1879, at Saratoga, N. Y.

H. CARRINGTON BOLTON,

*General Secretary.*

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## AMERICAN ASSOCIATION FOR

Dr. IN ACCOUNT WITH F. W. PUTNAM,

1877-78.

| | | |
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| *To Expenses of Nashville Meeting:* — | | |
| General Expenses at Nashville | $ 223 49 | |
| 250 copies Constitution and List of Members | 29 00 | |
| | | $ 252 49 |
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| Balance on hand to next Account | | 106 76 |
| | | $5,588 51 |

SALEM, Mass., August 13, 1878.

I have examined the above account and certify that

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## THE ADVANCEMENT OF SCIENCE.

PERMANENT SECRETARY. Cr.

| | | 1877-78. |
|---|---|---|
| By Assessments previous to the 26th Meeting . . | $ 279 00 | |
| " " for Nashville, the 26th Meeting . | 1,755 00 | |
| " " " St. Louis, " 27th " . | 462 00 | |
| " " " 1879, " 28th " . | 6 00 | |
| " Entrance Fee, Buffalo, " 25th " . | 5 00 | |
| " " Fees, Nashville, " 26th " . | 795 00 | |
| " " " St. Louis, " 27th " . | 10 00 | |
| Fellowship Fees . . . . . | 58 00 | |
| | | 3,370 00 |
| Publications sold, and Binding . . . . . | | 352 40 |
| Donation from Wm. B. Rogers . . . . . | | 100 00 |
| Express charge refunded . . . . . | | 2 00 |
| Life Membership commutation . . . . . | | 50 00 |
| Received from Treasurer, balance due Perm. Sec'y | | 1,714 11 |
| | | $5,588 51 |

the same is correctly cast and properly vouched.

HENRY WHEATLAND, *Auditor.*

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